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OF

MEDICINE AND SURGERY:

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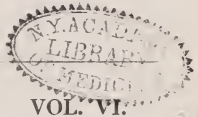

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MEDICINE AND SURGERY.

JULY, 1842.

ART. I.—*A History of the Improvements which Practical Medicine has derived from Auscultation; being the essay to which the Medical Society of Bordeaux awarded its prize in November*, 1839. By G. PEYRAUD, Doctor of Medicine of the Faculty of Paris, Corresponding Member of the Bordeaux Medical Society. *Quæ fundata sunt in natura, crescunt et perficiuntur; quæ vero in opinione, variantur, non augentur.*—Baglivi. Translated from the French for the Western Journal of Medicine and Surgery, by CHARLES A. POPE, M. D., of St. Louis, Lecturer on Anatomy and Surgery, Physician to the St. Louis Dispensary, &c., &c.

(NOTE BY THE TRANSLATOR.—A desire to be useful, and to employ the leisure occurring at the outset of a professional career, which might otherwise be less profitably spent, has prompted the present task.

To those physicians who are already acquainted with auscultation and the advantages which have resulted from it to practical medicine, this work will, I think, give a more clear, connected, and methodical idea of the improvements derived from the discovery of Laennec than any other yet published; while to such practitioners as are yet unacquainted with the art, and who, through ignorance and prejudice, undervalue its aid, it will, I hope, set forth arguments, and afford inducements, sufficient to incite them to acquire a knowledge of this means of explanation, which will best enable them fully to appreciate its utility.

By a glance at the contents, it will be seen that our author has surveyed the whole field of auscultation, not only as to its use in the diseases of the chest, but also in its application to cases of an entirely different character. He has, I think, treated his subject with rare ability, and the work is one of just and impartial criticism. As it is addressed to those already acquainted with auscultation, I take great pleasure in recommending to the attention of such physicians as are not yet familiar with physical examination, and who are distant from our large cities, and consequently deprived of access to extensive hospitals, *Gerhard on the Chest*, and particularly the last chapter, for facilitating them in acquiring a knowledge of the physical signs.

Relatively to the theories of the sounds of the heart, I may here mention that a new one has recently been started by M. Cruveilhier of Paris. He was called to see an infant, a few hours after birth, in whom an extraordinary malformation of the chest existed. The heart was situated fairly out of the thorax, from which it had passed through a circular opening in the upper part of the sternum, being thus as completely exposed as if the sternum had been removed, and the pericardium opened. M. Cruveilhier made many interesting and important experiments in this case, with the view of determining the seat and cause of the movements and sounds of the heart. They are too long to be reported in this place; a fuller account may be found in the Medico-Chirurgical Review (January, 1842, p. 193 et seq). He infers, however, from his observations, that "the two sounds of the heart have their seat at the origin of the pulmonary artery and aorta, and their cause in the clacking of the sigmoid valves *(du redressement des valvules sigmoïdes, préalablement abaissées)*; and that the second sound, which coincides with the diastole of the ventricles and with the contraction of the auricles, is the result of the closing together of the same valves folded back by the retrograde wave of blood."

With regard to the manner in which I have accomplished my task, I may say that no one is more sensible than myself, of its many imperfections; and those who are aware of the difficulties inherent to every translation, and the almost impossibility of giving the various shades of an author's meaning in a language other than his own, will be the first to grant their indulgence. I have endeavored always to give the sense in as plain and perspicuous a language as possible; but that some gallicisms will appear, I think more than probable).

When Baglivi wrote the lines which serve me for a motto, he established the surest principle for separating opinions founded in nature, from mere theories and vain hypotheses. Should one of these new ideas, such as are daily arising, appear in the scientific world, come from whatever quarter it may, from one of the masters of the art, or from some obscure member, let us not be misled by the clamorous admiration of complaisant friends, nor prejudiced by criticisms inspired by envy or springing from foolish precipitation. Let us wait, for time will quickly decide its worth. If this new idea be true, and founded in nature, it will gain ground despite the bitterest criticisms, and will finally become an established and incontrovertible truth. If, however, it be false and devoid of foundation, the exaggerated praises of those who have an interest in its propagation, will not sustain it. It will be

changed, altered, or modified by each of its partisans, who will think, in doing so, to mask its weak side, and it will at length fall into complete oblivion, whence it will never again be drawn, save by him who would give the history of the errors of the human mind. Indeed, examples would not be wanting to establish the truth of this principle; and, to cite only one, if Gall were to rise from his tomb, on which the weight of years as yet bears lightly, would he recognise in Phrenology, as it is now taught, the science which he thought to have founded? His successors have changed all that he did; the intellectual and instinctive faculties are no longer the same which he established; the places which he assigned them in the encephalon, have been appropriated to others: and perhaps, applying to himself the principle of Baglivi, he would suspect that he had put forth an hypothesis only: *Quæ fundata sunt in natura, crescunt et perficiuntur; quæ verò in opinione, variantur, non augentur.*

Such, however, is the principle which I boldly lay down as the touch-stone to prove the excellence of auscultation. It is known how this art was discovered. In 1816, Laënnec, whilst treating a young person for a disease of the heart, conceived the idea of exploring the beatings of this organ, by means of a few sheets of paper which he formed into a scroll, and was much surprised to hear them more distinctly than he had ever done before. He immediately felt the importance of this means of investigation, and saw in it the germ of a revolution in diagnosis, which would change the face of medicine, so far as thoracic diseases were concerned. He at once set about making a series of observations at the Necker Hospital, the result of which was the immortal work that has rendered his name so familiar. With what enthusiasm would not that same Baglivi have received it, who in 1696 so sorrowfully exclaimed: "*O quantum difficile est curare morbos pulmonum! O quantò difficilius eosdem cognoscere, et de iis certum dare præsagium! Fallunt velperitissimos, ac ipsos medicinæ principes. Tyrones mei, cauti estote et prudentes, in iis curandis, nec facilem promittite curationem, ut nebulones faciunt, qui Hippocra-*

tem non legunt."—(Prax. Medicæ, lib i, cap. xii, de Pleuritide.)

More than a century elapsed after this bitter and poignant reflection of the practitioner of Rome, and the state of things continued unchanged. The difficulty attending the diagnosis of diseases of the chest was still greater than that of their treatment, which, however, presented many obstacles and dangers. Laënnec's book appeared, and so complete and prompt was the revolution effected, that henceforward the proposition of Baglivi should be reversed, and it should be unanimously agreed that it is much less easy to cure diseases of the chest, than to recognize them.

Twenty-four years already separate us from that epoch, and auscultation still continues the most beautiful ornament of modern medicine; and the labors of the numerous authors who have followed Laënnec in the new path, have only confirmed the truth of his observations, the sagacity of his remarks, and the admirable exactness of the signs which he assigned to the diseases of the lungs. Auscultation has neither varied nor changed; it is precisely what Laënnec left it; for that indefatigable observer had brought it to such a degree of perfection, that there was nothing left for others but the hope of confirming what he had taught.

Will we not find in this astonishing perfection of so useful an art, the best and most unexceptionable proof, that it was really founded in nature, and not in a false hypothesis? What more natural, indeed, than to study the sounds which the respiration engenders in the organs where this function is accomplished? As these sounds cannot be modified without the function also, do not the modifications of the normal sounds, when well studied, become valuable signs in the diagnosis of the lesions which thus involve the function? This point once firmly established, all that remained was to collect a great number of facts, and especially to seek from pathological anatomy the confirmation of the relation which was supposed to exist between the modifications of the respiratory sounds, and the various degrees of the diseases that were observed. It is

this which Laënnec and his successors have done, and it is in some manner an inventory of the improvements which have resulted from it to practical medicine, that I am called on to make in answer to the wishes of the Bordeaux Medical Society.

This inventory I shall make scrupulously and conscientiously. Studying successively, and in as many distinct chapters, the different diseases of the lung and of the pleura, I will commence by showing what the labors of the different authors anterior to Laënnec had done for the diagnosis of those diseases. This exposition will be long; but it has seemed to me indispensable to the proper appreciation of the immense stride which auscultation has made in pathology on such difficult points; and the importance of this progress will be better set forth and become more evident, when we shall know in detail the errors into which the best minds have fallen, deprived of this valuable means of investigation.

Coming next to the diseases of the heart, I shall have a painful admission to make; but I shall makeit, for I do not think that an eulogium *quand même*, and absolute on all points, is demanded. I will say, then, that the results of auscultation applied to diseases of the heart have thus far been almost completely useless. Abnormal sounds have been heard, but they have not been explained, because a fixed starting-point, one admitted by all, has been wanting. The interpretation which has been given, has varied according to the idea entertained of the primary cause of the natural sounds of the heart; which furnishes us with a confirmation of the second part of the maxim of Baglivi: *Quæ verò in opinione, variantur non augentur.*

A chapter will be devoted to asthma and to angina pectoris, in which I shall demonstrate that auscultation has still rendered service to pathology, by proving that if, in certain cases, the material lesions coexisting with those diseases could be recognized, they could rarely explain the morbid phenomena which constitute them; a result extremely useful, although contrary to their *localisation*, inasmuch as it proves that there

is in those diseases a nervous element which gives them their essential character.

Finally, the last chapter will be devoted to a rapid survey of the various applications which have been made of auscultation to other affections than those of the chest, and especially to pregnancy.

I ought to premise, that all I shall say, will be equally applicable to mediate or immediate auscultation. The discussions concerning the superiority of one method over the other are now exhausted, and to renew them would be veritable nonsense. The results produced by them being identical, one ought not to be preferred to the other, but they should be combined together for the greater convenience of the person who practices them. Thus the naked ear will be directly applied, if it is desired, on the convex part of the back and on the anterior portion of the chest. Recourse will be had to the stethoscope for parts the conformation of which does not permit the approach of the ear, as the axilla, or with females, in whom modesty generally interdicts every other mode of auscultation. These are points now admitted by all, and quite beyond discussion; I shall not dwell longer on them.

CHAPTER I.—OF PLEURISY AND PNEUMONIA.

I shall speak of these two diseases in the same chapter, for prior to the time of Laennec, there was no differential diagnosis, and perhaps his most legitimate claim to universal admiration arises from his having assigned to each, symptoms as different as their anatomical characters. Let us inquire what was, before his time, the state of the science on this all important branch of pathology.

Cullen thus commences his chapter on *pneumonia or fluxion of the chest*: "It is my design to comprise under this head all the inflammations which affect either the viscera contained in the thorax or the membrane lining the internal surface of that cavity; *for no sign can serve to determine exactly the different seat of the disease, &c.*" (Pract. Med., transla-

ted by Bosquillon, chap. VI, §334). This is a frank avowal, but it was at that time the honest expression of the truth, as will clearly appear from a comparison of what nosologists have said on this subject.

I know not indeed whether I ought to analyse the symptoms that Cullen attributes to *fluxion of the chest*, since we are so well apprised that under this title he includes two diseases now so distinct. These symptoms are fever, difficult respiration, pain in some part of the chest, and cough, which he says is sometimes dry, sometimes accompanied by expectoration varying in consistence and in color, but in which streaks of blood are frequently observed. These symptoms are somewhat vague; a pain in some part of the chest is not always pleuritic; but it is certain that added to fever, difficult respiration and bloody sputa, it is pretty characteristic of a phlegmasia of the organs contained in that cavity. But what part do the lung and pleura bear in this inflammation? An answer cannot be expected from the author when he adds: "There is but little reason, therefore, for designating this disease by different names taken from the part which may be supposed to be particularly affected. The term *pleurisy* answers in all cases, and it has been very improperly restricted to mean the inflammation commencing in that portion of the pleura which lines the ribs and which is particularly affected. I do not doubt that this really happens, but at the same time I suspect that it is a rare occurrence." (Op. cit. §341.)

The translator and commentator of Cullen, Bosquillon, in endeavoring to supply his deficiency, thus establishes the differential diagnosis of pleurisy and peripneumonia.

"*Characters of peripneumonia.*—The pulse is not always hard in this inflammation; it is sometimes soft; the pain in the chest is dull, the respiration is invariably difficult, and often can be performed only when the trunk is in the erect posture; the face is swollen and of a purple color; there is a cough, commonly humid, often bloody."

"*Characters of pleurisy.*—In pleurisy the pulse is hard, the side is generally affected with a pungent pain, which is in-

creased during inspiration. The patient lies with difficulty on his side, and the cough is very painful, at first dry and afterwards moist, often bloody." (Op. cit. note by Bosquillon, vol. i, pp. 376 and 377).

The characters here assigned by Bosquillon to pleurisy and to peripneumonia are not without importance. Thus the hard pulse has been indicated as properly belonging to pleurisy, by several authors, and among others by Baglivi, who thus expresses himself: "*Si vis cognoscere pleuritidem, præcipuam curam in natura pulsus cognoscenda reponito: pulsus durities est signum fere infallibile omnium pleuritidum; et dum obscuræ sint pleuritides, vel aliis complicatæ pectoris morbis, si duritiem (id est nimiam arteriæ tensionem vibratonemque), in pulsu deprehenderis, quamvis reliqua earum signa non adsint, pro certo habeas patientem laborare pleuritide, &c.*" (Op. cit. lib. i, cap. ix, de Pleuritide).

The softness of the pulse in pneumonia has likewise been pointed out by most authors, and Dehaën particularly gives us a very ingenious explanation of it. But a distinctive character of the two diseases, far more essential than the state of the pulse, especially as it is more easily appreciable, is the pungent pain, "sometimes limited to a part which may be covered by the finger." This local and fixed pain essentially distinguishes pleurisy from pneumonia. The difficulty of lying on the affected side, is also one of the characteristic signs of pleuritic inflammation; but is it always very easy to distinguish between it and the difficulty of respiration in other than the upright posture which Bosquillon notices in pneumonia? Lastly, I ask, are these characters, however just in themselves, sufficient to guard the practitioner against the commission of error?

A little further on, Bosquillon points out *vomica* and *empyema* as ordinary results of pneumonia. A vomica may be recognized by the continuance of the cough and dyspnœa, the impossibility of lying on the affected side, and the development of hectic fever. In case of empyema, the cough, difficulty of respiration and lying in the recumbent posture are

present, there is likewise hectic fever; *the patient often feels at the same time a kind of fluctuation produced by the liquid contained in the chest.* With the exception of this last symptom, all the others enumerated by Bosquillon have very little value in the diagnosis; and does this last always exist? When Bosquillon pointed it out, was it from clinical facts that he did so, or was he not unwittingly influenced by the remembrance of the Hippocratic succussion? However this may be, the sign but very rarely exists, since we shall hereafter see in speaking of pneumo-thorax, that, in order to the success of the Hippocratic succussion, there must be in the pleura both air and pus at the same time, the only circumstances under which the latter can be agitated and made to give out the noise of fluctuation mentioned by Bosquillon.

Finally, he adds, *the signs of hydro-thorax are superadded to these symptoms*, and the former he enumerates as follows: "The signs which characterize hydro-thorax, are dyspnœa, pallor of the countenance and œdema of the extremities; the patient experiences great difficulty in keeping the recumbent posture, he starts in his sleep and complains of palpitations. There is a perceptible fluctuation in the chest." (Cullen, op. cit., note by Bosquillon, vol. iii, p. 293.) Apart from the fluctuation of which I have just been speaking, this symptomatology is exact; but it is insufficient. It offers no means of ascertaining in which side of the chest the effusion exists, nor its extent. Moreover, there is not one of these symptoms which is not in like manner produced by a serous effusion in the pericardium; and a diagnosis founded on these data alone, without seeking new elements elsewhere, would necessarily be uncertain, imperfect, and unsatisfactory to all.

Sydenham has treated of pleurisy and pneumonia; but he regards both as the result of an inflammatory state of the blood: "When the febrile matter throws itself on the pleura or the intercostal muscles, it ordinarily happens in the commencement of the fever, the morbific matter being yet crude, and not having had time to undergo the necessary concoction and preparation, to be evacuated by the suitable outlets.

The common cause of this occurrence is the improper use of heating remedies." (Sydenham, Pract. Med., translation of Jault, p. 249.) Of course he attached but little importance to the recognition, during life, of the development and even the precise seat of anatomical alterations, which he regarded as the consequence only, and not as the producing cause of the fever. However, he clearly indicates the painful stitch in the side as one of the essential signs of pleurisy; and his translator, M. Jault, endeavoring in a note to give precision to the signs by which the existence of adhesions between the lung and corresponding pleura is recognized, thus expresses himself: "The symptoms which enable us to judge most certainly whether there be adhesion, is when the patient can lie on but one side, without pain and with a tolerably easy respiration. The adhesion is always on the side on which the patient lies easily." (Op. cit.. note by Jault, p. 248.) M. Jault supports this opinion by two reasons: 1st, that when the patient lies on the sound side, the lung tending by virtue of its weight to separate from the costal pleura, the adhesions are put on the stretch; 2d, that in the decubitus on the healthy side, the sound lung cannot supply the unsound lung in its functions. These signs are very rational, but they can only authorize the suspicion of adhesions between the lung and the corresponding ribs; they are not sufficient to render their existence certain. We know that in investigating the effects of these adhesions and the characteristic signs by which they are revealed, Laennec discovered that their constant effect was to diminish that side of the chest which had been the seat of the pleurisy, the consequences of which they were; this furnished a pretty characteristic sign. But to dwell longer on, this subject would be leaving the question which the *programme* has so precisely limited to the improvements that auscultation has made in the diagnosis and treatment of diseases of the chest. I continue my review of the nosologists who preceded Laennec.

The two diseases we are now considering are frequently discussed in the *Practical Medicine* of Stoll. But in his con-

stant and almost exclusive eagerness to pursue the bile, and the crude and acrid matters of the primæ viæ, the practitioner of Vienna is much more frequently engaged in the search of signs to prove their complication with a bilious state, than in looking after the characters that might serve to distinguish them. Moreover, what he says of pleurisy and bilious pneumonia observed in April, 1776, proves that he regarded them as two different degrees of one and the same malady. "This observation," he says, "is important in the diagnosis: that in bilious pleurisy and pneumonia the pain is rarely increased by coughing, or during inspiration; whilst those who are attacked with true inflammatory peripneumonia, can neither cough nor respire without a violent pain in the side. Besides this, the sputa is rarely tinged with blood in bilious pleurisy, unless it be sufficiently violent for the efforts of coughing to bring away a little blood." (Stoll, Pract. Med. translated by J. Terrier, vol. i, p. 65.) Elsewhere, speaking of the importance of the redness of the face, in distinguishing false pleurisy from true inflammation of the lungs, after having examined passages from Baillon and Hippocrates on this subject, he arrives at this conclusion: "that persons are seen affected with pneumonia, whose lungs are attacked by a true and violent inflammation, although the face be very pale, &c." (Op. cit., vol. i, page 68.) From all this does it not appear very evident, that, in his view, pleurisy and pneumonia are the same thing? are not the two words used indifferently by him, at random, so to speak, without any motive to justify the employment of one or the other? Leaving very soon the description of these diseases, Stoll, in the remainder of the passage which I am analysing, busies himself in pointing out the indications for tartar emetic, a remedy which he is known to have used almost exclusively, although he was far from regarding it as contra-stimulant; and I can legitimately conclude that he has in no wise advanced the diagnosis of these two diseases.

Morgagni has devoted two letters (the 20th and 21st) of his immortal work, *De sedibus et causis morborum*, etc., to the study of pleurisy and pneumonia, under this title: *Of pain in*

the chest, sides and back. The illustrious founder of pathological anatomy had opened too many dead bodies to be in danger of falling into similar errors respecting the nature and seat of these two phlegmasiæ. He is seen, in the remarks which always follow each history, endeavoring to show that the part where the patient had felt the pain, is invariably the same at which the lung is found adherent to the pleura; that in cases of peripneumonia, the pain is always gravative, whilst in cases of pleurisy it is more acute and pungent, *on account of the great number of nervous fibres which terminate in the pleura.* (Op. cit., translated by Désormeaux, vol. iii. page 305). However, when the real opinion of Morgagni on the nature of these two phlegmasiæ, is sought for, it will be found that he does not think they can exist separately, except it be to a very slight extent. We find him, indeed, in his 21st letter, accumulating authorities to prove that a pleurisy cannot cause death, without the phlegmasia extends itself to the lung. "Hippocrates, in his book *De locis in homine*, has positively placed in the lung not only the seat of peripneumonia, *but also that of pleurisy. . . . . Cœlius Aurelianus* teaches, with Praxagoras, Herophylus and Euryphontes of Gnidus, that the lung *is the part that suffers in pleuritic patients.*" Further on he cites the observations which Hoffman says were made in the hospital Santo Spirito, at Rome, by *Servius, on three hundred patients affected with pleurisy,* "*among whom he constantly saw one lobe of the lung putrified and filled with matter, whilst the pleura absolutely had no appreciable lesion, or was but slightly altered in any way.*" Still further on he cites Riviere, in whose *Sepulchretum* these words are found: "*The very violent pleurisies, which ordinarily induce death, degenerate most often into peripneumonias;*" and Triller, a most worthy physician, who assures us that generally, "*in a true pleurisy there exists not merely an affection of the pleura, as has been heretofore foolishly believed, but likewise a simultaneous alteration of the very substance of the lungs, as anatomy, the only light of medicine, clearly teaches;*" and lastly, the great anatomist Haller, who says, "*that he has never thought an inflammation of the pleura alone killed any man.*" (Op. cit.,

vol. iii, p. 482 and following.) He had already in his 20th letter quoted Coiter, who remarks that *peripneumonia is one of those diseases accompanied by dropsy of the chest.* (Op. cit., vol. iii, p. 339).

Thus then, according to the illustrious professor of Bologna, pleurisy and peripneumonia scarcely ever exist separately. He knows their inflammatory nature, he no longer considers them as an accumulation of febrile matter upon the chest, or as a reflux of acrid and bilious matters from the stomach to the thoracic vessels. These errors had to disappear before the broad light which his ever celebrated labors shed on the seat and nature of diseases. He did not finish his work, however, but still left something to be gleaned by his successors in the vast field of pathological anatomy, which he cultivated so successfully. We shall after a while see how much more distinct the labors of the moderns, and above all of Laënnec, have made the special lesions peculiar to each of these two maladies, at the same time that auscultation will furnish us the means of recognising in the patient the separate existence of these lesions.

To continue our review of authors:

I have shown that Cullen, Sydenham, Stoll and Morgagni considered pleurisy and pneumonia as only one and the same disease. Did I not fear uselessly to prolong this work, it would be easy to add to these venerable names in the science, those of other authors not less worthy of esteem. Thus Sarcone devotes a pretty long chapter of his *History of the epidemic at Naples in* 1764, to prove that pleurisy and pneumonia are the same thing (op. cit., translated by Bellay, vol i, p. 126 and following); and we find in the Memoirs of the Academy of Sciences for 1789 an observation by Portal, *which proves that pleurisy is not essentially different from pneumonia.*

However, an opinion so erroneous did not pass without contradiction, and in vol. ix of the *Ratio Medendi*, Dehaën is seen to contend warmly with Boërhaave, in opposition to Hal-

ler and Tissot, that pleurisy and pneumonia are two distinct diseases.

Leaving these personal disputes, let us see how Dehaën in another of his works endeavors to characterise each of them.

He first asks himself what is the pathognomonic sign of pleurisy? Is it the stitch in the side? No, for hysterical patients have pains in the side, which anti-phlogistics do not abate. Is it the difficulty of respiration? No, for this is also observed in asthmatic patients. Is it fever? But all acute diseases produce that. Lastly, is it the hardness of the pulse? But the pulse is hard in phrenitis. No one, then, of these isolated symptoms, essentially characterises pleurisy; but all four united, especially if the stitch in the side be increased by inspiration, authorises us to announce its existence.

Passing to peripneumonia, Dehaën enjoins redoubled attention, as a mistake is easily made; and again he asks himself what is its pathognomonic sign. Is it the pain? But nothing is more common than peripneumonia without pain. Is it the difficulty of respiration? But we observe this in violent fevers. Is it the softness of the pulse? No, for the pulse presents this softness only when the disease is far advanced and death is imminent. He then states that the pulse is developed in peripneumonia, but that it presents at the same time something wave-like and soft, and that this softness increases in proportion as the lung becomes less and less permeable to the blood, a smaller quantity of which reaches the aorta; and he finally arrives at this conclusion, which I transcribe literally: "*Sed tamen febris initio magna, respiratio multum læsa, tussis, anxietas, facies rubor, urina cruda, peripneumoniam adesse concludere faciunt.*" (*Prælectiones pathologicæ*, vol. ii, p. 478.)

It is impossible not to admire the sagacity with which Dehaën discusses the value of the signs of these two affections, which the spirit of his time tended to confound and regard as only one. We recognize the experienced practitioner in the minuteness of the remarks which he makes on the pulse in pneumonia, remarks the truth of which daily experience

still shows. But I ask, has he succeeded in solving the problem, and are the signs which he indicates sufficient to distinguish pleurisy, more than pneumonia? Such is the uncertainty in which have terminated the labors of so many celebrated pathologists! either to make of these two phlegmasiæ one disease, or to be unable to distinguish the one from the other, admitting, too, the possibility of their separate existence. Let us not be in haste, however, to throw aside the results of their studies and the fruits of their observations; let us rather inquire if it was possible for them to have done better, having as guides in their researches general symptoms only, and possessing no means of appreciating the physical signs which have since aided so much in the *localisation* of diseases of the chest.

At length, in 1808, Corvisart published a translation of Avenbrugger's *Treatise on Percussion*, with comments. The original work was printed at Vienna in 1763, and afterwards fell into total neglect, Stoll and Van Swieten being the only authors that mention it. The success of this mode of investigation has been constantly increasing since its revival, and it has now become the necessary companion of auscultation.

I find but little in this work relative to the diseases which we are considering. I scarcely think that Avenbrugger ever tried percussion of the chest in pneumonia or in pleurisy; it would have required a good deal of boldness and even temerity, at that period, to have added even the slight pain of percussion, to the pungent pain of pleurisy. Such trials are not generally made, except in cases where we know beforehand the result we seek, and rarely from mere curiosity; and hence all that Avenbrugger has accumulated in his very little treatise relates solely to chronic diseases of the chest, or of the heart. I shall avail myself of this in due time.

Before leaving this author, however, I will note the signs which he lays down for recognising dropsy of the chest: "Besides the general symptoms just enumerated (they are those indicated by the other authors), we observe that the affected side, if it be entirely full of water, is weaker and appears less moveable during inspiration; when it is percussed, no sound is

elicited from any part, but if it be only half-filled with water a louder sound will be obtained in that part above the liquid. In this case the sound varies according to the position of the patient, so that it will follow the direction of the level of the liquid." (Op. cit., translated by Corvisart, p. 374). In these few lines, Avenbrugger makes an immense step in the diagnosis of effusions; and the reader will observe that his method indicates the height of the effusion, and the mode of ascertaining whether it be a liquid which fills the chest, or solid matter, such as the swelled lung; rendered impermeable to air; for in the latter case the change of position does not displace the seat of dulness.

Pinel was acquainted with the treatise of Avenbrugger when he wrote his *Nosographie philosophique*, and he has not neglected to apply this method to the diagnosis of the effusion which almost always accompanies pleurisy. He notices the dull sound on the side of the effusion; and he adds to it all the other symptoms which we have seen imputed by nosologists to dropsy of the chest. As to pneumonia and pleurisy without effusion (if there be any such thing), he distinguishes them by nearly the same symptoms which we have analyzed in Dehaën.

At last Laënnec appears, and the old question of the separation or the confusion of the two diseases is forever decided: for this purpose he begins by giving the precise anatomical characters belonging to each.

Inflammation of the pleura is always accompanied by an exhalation on the internal surface of the membrane. This exhalation, which Laënnec regards as a mode of suppuration peculiar to serous membranes, is present from the beginning of the inflammation, and results in the formation of false membranes and a sero-purulent effusion. False membranes form those whitish patches which cover the whole inflamed part of the pleura, and the external surface of the lungs. Sometimes more consistent, they form bands which pass from the lung to the pleura traversing the effused liquid. Susceptible of organisation, blood vessels may then be seen developing them-

selves in their interior, and they constitute those adhesions of the lungs to the ribs, so frequently observed in individuals who have suffered with pleurisies at remote periods.

As for the effusion, its nature varies extremely. Generally yellow, whitish, mixed with albuminous flocculi which are only detached false membranes, it has been compared to whey. Its quantity is likewise subject to much variation: the more intense the inflammation, the more abundant are the false membranes, and we then find but a few ounces of effused serum; when the inflammation is less, and in feeble lymphatic subjects, false membranes are more rare, and the effusion may become very great and be confounded then with hydrothorax.

The parenchyma of the lung remains intact when the pleurisy is simple, save that it is pressed back against the mediastinum, and, in consequence of the compression which it experiences, becomes less crepitant than in the natural state.

Pneumonia presents in its anatomical characters, three well marked stages: *engorgement, hepatization and purulent infiltration.* An *engorged* lung is heavier, more compact, and less crepitant than in the healthy state; it retains the impression of the finger like an infiltrated limb; it is filled by a liquid which flows out abundantly on a section being made, mingled with globules of air which render it frothy; the red pulmonary tissue is then seen, retaining, however, its alveolar form.

The word *hepatization* indicates sufficiently well the condition of the lung in the second stage of inflammation; harder, heavier, no longer crepitating under the finger when pressed, it acquires a consistence analogous to that of the liver; internally it presents a very deep color varying from violet gray to blood-red; nothing flows out on making an incision, but by scraping the cut surface with the scalpel a sanguinolent serum is obtained, thicker and more turbid than that of engorgement; lastly, the pulmonary substance presents nothing cellular to the eye, but exhibits a granular appearance

which is the peculiar anatomical character of pulmonary inflammation, and which exists only in this disease. Finally in the third stage, that of *purulent infiltration*, the pulmonary tissue is much less hard and consistent than in the second, and assumes a pale yellow color, analogous to that of straw. This color is owing to the infiltration of pus, which oozes out more or less abundantly when the lung is incised; the granular appearance of the parenchyma, which constitutes the essential character of the second stage, disappears in proportion as softening is effected. When the lung contains much black pulmonary matter, which is common in adults and old persons, the pus and infiltrated lung assume an ash-gray color, which many authors and especially M. Andral have noticed under the name of *gray softening* (*ramollissement gris*).

The reader will pardon me for having touched slightly on the anatomical characters of the two diseases under consideration; it was indispensable to the proper estimation of the difference existing between the two, and to show the great error of those who have considered them one and the same disease. Let us see now by what signs Laennec distinguishes each during life.

I will not here repeat the enumeration of general symptoms drawn from the fever, the pain and the nature of the expectoration; I have already discussed their value at sufficient length, in speaking of the nosologists who preceded Laennec, and I think I have demonstrated, that alone, they do not give to diagnosis the certainty which it should possess. Percussion of the chest produces a dulness nearly equal in the two phlegmasia, nor does this sign furnish infallible data for judging of the nature of the disease. Pretensions have been made to draw differential signs for each of these diseases from percussion, by the difference of the sounds obtained when the patient varies his position; but it was forgotten that in order for the liquid to be displaced by every movement of the patient, the chest should be partly empty, whereas it is always exactly filled by the organs which it contains. The only

sign at all important furnished by percussion, is the extent of the flatness in pleurisy; in a short time a whole side of the chest becomes flat, whilst in pneumonia, the chest loses its sonorousness by degrees, and the flatness remains for a long time circumscribed. But let us turn to the results furnished by auscultation.

In pleurisy, a liquid being interposed between the parietes of the chest and the lung, 1st, the respiratory murmur ceases to be heard over the whole of the affected side, unless, however, it be in an extent of about three fingers' breadth along the vertebral column (in which place the lung is found, compressed on all sides by the effused liquid); 2d, if the patient be made to speak, the voice assumes a peculiar trembling character, which Laennec has named *egophony*.

In pneumonia, 1st, the essential sign from the beginning is *crepitation*; it occurs in no other affection than hemoptysical engorgement (which is known, indeed, to be a real species of pneumonia, of which I will speak further on, under the name of lobular pneumonia); 2d, when the second stage of pneumonia commences, the crepitant râle disappears, to be replaced by a bronchial respiration, which proves that the air does not penetrate into the pulmonary vesicles, but only into the bronchiæ, the parietes of which are rendered thicker by the hepatized parenchyma; 3d, when the patient speaks, the voice reaches the ear with a very strong resonance, without trembling, but not so directly as in pectoriloquy; 4th, when suppurative softening takes place, the mucous râle is heard.

Finally, *in pleuro-pneumonia*, the combination of the symptoms of the two diseases will prove that the phlegmasia involves at the same time both the pleura and the lung.

The different abnormal sounds which I have just indicated, are not always very distinct, and often it is not without difficulty that some of them are distinguished from others. Thus, in pleurisy, it is not always easy to perceive egophony and to distinguish it from bronchophony; each, in fact, consists in a resonance of the voice, through the parietes of the thorax, which, much less direct and much less distinct than in pectoril-

oquy, is sometimes equally strong in both. A similar cause indeed produces them, viz: thickening of the walls of the bronchiæ, which in bronchophony arises from hepatization of the lung, and in egophony from effusion; the only difference consists in the trembling character given to egophony by the fluid which the voice traverses in passing to the ear of the observer. This character is the more easily detected, as one becomes more practised in auscultation. But in the most embarrassing cases, and when there is the greatest difficulty to determine which it is that is heard, error may be prevented by reflecting: 1st, that bronchophony is never heard but in a circumscribed space, whilst egophony is heard over the whole, or at least the greater part of one side of the chest, a space which the former could not occupy without the severity of the general symptoms presented by the patient evincing that it was not a simple pleurisy that affected him; 2d, that the seat of bronchophony is invariable, whatever be the position of the patient, whilst, when egophony occupies a part only of one side of the thorax, which in that case is always the most dependent, it can be displaced by a change of position; 3d, and finally, that it is impossible to hear at the same time, and in the same zone of the lung, the natural respiratory murmur and bronchophony, because if the pulmonary vesicles be still permeable to air, the parenchyma no longer presents that indurated condition necessary for the voice to resound in the bronchiæ as in tubes having solid walls. Now, in cases where egophony is perceived most distinctly and over the most extensive surface, the respiratory murmur is always heard at the same time in a space of about three fingers breadth along the vertebral column, the lung being forced back there by the effusion.

When the effusion is considerable, egophony disappears, but the dullness increases, the affected side of the chest is dilated, and this dilatation is often perceptible to the eye; sometimes the diaphragm is pushed down by the effusion, producing a considerable prominence of the abdomen, which cannot be touched without increasing the oppression; lastly, the

persistence of the respiratory murmur along the spine will guard against all error. When the effusion diminishes, the egophony reappears; in which case it is always at the superior part of the thorax that it is first heard; this might be accounted for by saying that it is more perceptible at the surface of the effused liquid. But the better reason is, that the lung, becoming more easily dilated in proportion as the effusion diminishes, is always found separated at its summit from the walls of the thorax by a thinner layer of liquid than at its base. Nevertheless it should be stated that this returning egophony is not always heard, as when long-standing membranous bands hinder the lung from resuming the degree of expansion which is natural to it in the healthy state.

Pleurisy may be double: in this case percussion affords no information, any more than the inspection of the chest. But the complete absence of the respiratory murmur in both sides of the thorax, except along the spinal column, would prevent any mistake, if for the most part the formation of an effusion in the side previously healthy, were not a phenomenon of the state immediately preceding death.

Finally, in those very rare cases where pleurisy exists without effusion, it will be recognized by the grating sound (*bruit de frottement*) produced by the false membranes lining the pleura and the corresponding part of the lung. And as there is always pneumonia at the same time, the symptoms of the latter will be found constantly predominating over those of pleurisy.

The analysis of the stethoscopic signs of pneumonia will not exhibit fewer proofs of their absolute certainty than those of pleurisy have presented.

I have said that the essential sign of pneumonia was the *crepitant* rhonchus, the name of which was suggested by its resemblance to the noise made by salt, which decrepitates when submitted to a moderate heat. This rhonchus is heard in pneumonia only (it matters not whether general or circumscribed); it is therefore justly regarded as its pathognomonic sign. It occurs in the pulmonary cellules, which are filled with

a liquid, but are still traversed by the air. M. Andral, from consideration of the fact, which is incontrovertible, that the crepitant rattle results from a mingling of air and liquid, observes that the mucous rhonchus and gurgling are produced by the same cause. As a proof of it, there is a certain number of cases in which it requires a very practised ear to distinguish crepitation from the mucous rhonchus; and a very great number in which it is extremely difficult to distinguish the latter from gurgling. Reasoning thus by analogy, M. Andral concludes that these different sounds, arising from an identical cause, present differences in consequence solely of the size of the cavity in which they occur. Thus, gurgling is heard in great excavations, the mucous rattle in the large bronchiæ, and the crepitant rhonchus in the extremities of the latter, and especially in the pulmonary vesicles. (Andral, *Clinique Médicale*, vol. 1, p. 524). These observations of M. Andral are very just, and clearly establish the seat of crepitation. But in the production of this phenomenon, one thing must be considered, besides the capacity of the place where it occurs: the nature of the liquid which fills the pulmonary vesicles plays a conspicuous part. Dr. Spittall, of Edinburgh, author of a Treatise on Auscultation, has made an experiment which proves that the less viscid this matter is, the more distinct is the crepitant rattle. He put equal quantities of liquids differing in density and tenacity into bottles, and, after agitating them, applied them in succession to the ear to hear the crepitation produced by the bubbles of air which rose and burst on the surface. The degrees of crepitation furnished by this means, differed with different liquids, and those which produced a crepitation the most resembling that of pneumonia, were *serum* and urine. These experiments prove that, in the onset of pneumonia, it is not the matter expectorated which fills the pulmonary vesicles and the extremities of the bronchiæ, since the more distinct the crepitation is, the more serous is the liquid which fills the vesicles. Now, the expectoration of pneumonia being always viscid, it must necessarily be produced by the upper bronchiæ and throat.

Crepitation is heard in the engorged points only: it therefore furnishes an infallible means of ascertaining, within a few lines, the whole extent of the engorgement, and of telling with an almost mathematical precision what part of the lung is attacked, and what part is still healthy. What means of diagnosis had ever furnished such precision as this?

When pneumonia passes from the first to the second stage, that is, when hepatization succeeds engorgement, the crepitant rale disappears; but then the flatness on percussion, the absence of the respiratory murmur in a circumscribed point of the thorax, and the existence of bronchophony in the same point allow no hesitation concerning the nature of the complaint, especially when added to a viscid and bloody expectoration, which is so peculiar to pneumonia that Laënnec proposed to name it *pneumonic*. I will not repeat what I have said above, of the means of distinguishing bronchophony from egophony: its principal characters are known to be its existence in a point ordinarily circumscribed, which does not change with the position of the patient, and its want of trembling. Let us add that, in those parts of the lung not invaded by inflammation, it ceases to be heard, and the respiratory murmur is more clearly perceived the greater the distance from the hepatized point.

The resolution of pneumonia, after it has reached the stage of hepatization, is announced not merely by the diminution of the general symptoms and the change in the nature of the expectoration, which becomes more and more white or yellowish, and in which blood is less and less perceptible, but also by the return of the crepitant rale, mingled with the murmur of pulmonary expansion, which becomes more marked from day to day until it finally exists alone. The return of crepitation in the decline of pneumonia, and of egophony in the diminution of a pleuritic effusion, have the great advantage of affording the practitioner the material proof of the success of his treatment, and of making it impossible that he should be misled by a deceptive mildness and amendment in the symptoms, such as is sometimes seen immediately preceding the catas-

trophe. At the same time, the observant physician finds in them a proof that our diseases abate by returning through the same stages which they presented whilst advancing; and that an engorged organ, undergoing resolution, does not differ physically from what it was when the engorgement was in progress, only that the vital force acts in an inverse mode: in the onset, by accumulating disorganizing materials; and in the decline, by taking up those materials again by means of absorption, leaving the organ in its primitive and normal state.

Under the name of *lobular pneumonia* a particular species of inflammation of the lung has been described, in which the engorgement is limited to one or more central nuclei which sometimes scarcely attain the size of a walnut. In this case is it always easy to discover crepitation? Authors are divided on this point. Laennec affirms that he always found it, and it is certain that the more experienced one is in the art of auscultation, the more rare do difficult cases become; and the art, therefore, should not be made responsible for the unskilfulness of those who practise it. In the case of which we are speaking, there is a combination of crepitation, which is heard in a deep and circumscribed spot, with the respiratory murmur, heard more superficially, but with its normal type and only a little more rude or puerile. When lobular pneumonia passes to the second stage, there is likewise a simultaneous existence of the respiratory murmur and bronchophony. If the central pneumonia increase and extend towards the surface of the lung, the respiratory murmur diminishes more and more, whilst bronchophony becomes nearer. And it is well to remark, that lobular pneumonia is a disease in which an error in diagnosis offers the least danger; for it is not a serious affection, and becomes so only when it has a tendency to invade considerable portions of the lung, and then it will be easily recognised by the stethoscopic signs already pointed out.

There is an affection, but little studied before the time of Laennec, the stethoscopic signs of which are slightly blended with those of pneumonia, and consequently it is sometimes

difficult to be distinguished, the more so as it frequently follows the latter disease. I mean *œdema of the lung*. The results of auscultation in this affection, are a diminution of the respiratory murmur, and a crepitant rhonchus the formation of which is naturally explained by the infiltrated state of the lung, which nearly approaches what we have described under the name of *engorgement*. But in *œdema*, the crepitant rale is much more humid than in pneumonia, for which reason it has been called *sub-crepitant*. Then, the general symptoms are not the same: the flatness is generally less than in pneumonia, because the lungs always contain air mixed with serosity, which does not happen in pneumonia; the expectoration is more abundant than that of the latter disease and differs from it essentially, as it is completely colorless, analogous in aspect and consistence to the white of egg beat up with water. Lastly, though the dyspnœa is generally so great as often to simulate that of asthma, which is owing to both lungs being generally infiltrated at the same time, still the symptoms of reaction are wanting, there is no fever and but slight cough, which ought necessarily to preclude the idea of pneumonia.

Such is the new symptomatology which Laennec has not substituted for the old, but added to render it complete and supply its deficiencies. It will now be easy for me, after this exposition, to enumerate the improvements which it has caused in the art of diagnosis.

Thus, 1st, auscultation has at length decided this great question, which pathological anatomy alone had not been able to solve; whether pleurisy and pneumonia are or are not two distinct diseases. And let it not be said that this distinction is useless to therapeutics! for who will dare say that an acute pleurisy should be treated absolutely in the same manner as an acute pneumonia? Would not the difference in seat necessarily lead to a modification in the therapeutic means? Who does not call to mind this essentially practical distinction which has been made between local and general sanguineous evacuations, accordingly as they are made for the

phlegmasia of a membrane or for that of a parenchyma?—that in the latter, venesection alone is capable of effecting a diminution of the hyperemia of which a parenchymatous organ is the seat; whilst in the phlegmasia of serous membranes, bleeding from the capillaries has a much more prompt and certain derivative action. When after the acute stage has passed, and revulsives must be thought of, will it not be of the highest utility to know the precise seat of the disease, so as to decide upon the proper place for their application? will not the indications vary according to the aspect which the disease may take? and will a pleuritic effusion be treated by the same remedies that we employ in a pneumonia, the resolution of which is accomplished slowly? All these things are so evident that there is no need of further remarks to make them understood.

2d. In enabling the progress of the two diseases we are considering, whether favorable or otherwise, to be followed, auscultation has rendered an immense service to the practitioner, who can thus judge of the effect of the remedies that he employs. I have already said, that the general symptoms are often deceptive, and sometimes have an apparent mildness which induces a security fatal to the patient. But auscultation properly applied, nearly assimilates the treatment of pleurisy and pneumonia to that of a wound, the daily modifications of which can be ascertained by the eye or the fingers of the surgeon.

3d. Auscultation, by enabling the precise seat of pneumonia to be determined in all cases, and by showing, within a few lines, the extent of the parenchyma which it invades, has disclosed this curious fact, which pathological anatomy alone would not have shown so well, nor so completely, that pneumonia generally begins at the base of the lungs, that it rarely commences at the summit and then is of a more serious character, as it is almost certain to involve the whole lung. And this observation, curious in itself, has furnished a very powerful argument to the opponents of that erroneous theory, which makes pulmonary phthisis a consequence of

chronic pneumonia. In fact, tubercles are always developed in the first instance at the summit of the lung, as we shall see in treating of that disease, whilst pneumonia always begins at the base. Does not this difference alone prove already that the nature of the two diseases is not identical?

4th. In pointing out the physical signs of effusion, an almost constant result of pleurisy, Laënnec has rescued surgeons from all chance of error hereafter in the operation for empyema; for it is evident, that, whatever be the nature of the effused liquid, serum, pus, or blood, the signs which indicate the presence of a fluid in the cavity of the chest do not vary. It is fearful to think how uncertain, before the discovery of auscultation, were the symptoms from which was deduced the indication for thrusting the bistoury through the parietes of the chest. In the patient operated on by Morand, and whose history he has inserted in vol. ii. of the Memoirs of the Royal Academy of Surgery (p. 545), this celebrated surgeon declared the necessity of empyema from the following symptoms only: fever, insomnia, pains in the head, in the neck, and in the whole epigastric region, principally in the left hypochondrium and in the chest of the same side; œdematous swelling of the whole left side of the body, suffocation to such a degree that the patient could scarcely move, or even spit or speak; impossibility of resting otherwise than on the back, *slightly inclined forward* (Morand doubtless intended to say *seated and sitting up in bed*); frequent syncope. Morand, assisted by Moreau and Louis, who, it appears, were not so well convinced as himself of the presence of pus in the thorax, made an exploring puncture at the place selected on the left side; and the issue of a stream of serum confirmed the opinion which he had formed. But in thus deciding the nature of the complaint, was Morand guided by the mere analysis of symptoms only? Did he not evince that species of presumption so natural to a superior man who trusts to his genius? He guessed well; but how many have been deceived! and how often has a useless and therefore dangerous operation been performed on a chest without effusion! We read in Dionis, that a surgeon who was oth-

erwise skilful, operated for empyema on the Duke de Mortemart, and found nothing in the chest. "A circumstance nearly similar to this," adds Dionis, "happened at Versailles in 1703, to one of the surgeons of the king. M. Helvetius came to see one Batteville, upholsterer to the king, who had been ill for a long time, and was complaining of a pain in the right hypochondrium; having touched the spot, he thought there was matter, and advised the surgeon to make an opening, which he instantly did; nothing followed the incision, and the patient died of the operation." (Cours d' opérations de chirurgie, by Dionis, vol 1, p. 435).

The contrary error is more frequent, and though less pernicious in itself, it is always injurious. In fact, examples are common of patients who have been found on dissection to have enormous pleuritic effusions, the existence of which had not been suspected during life. Professor Boyer cites from Ledran a remarkable example: "A patient was brought to La Charité, in whom, two days before, a deep abscess under the angle of the right maxillary bone had been opened. The third day the suppuration was suppressed, the patient had a chill, and felt a smart pain in the left side of the chest, with considerable oppression. The great symptoms or rather signs which denoted that suppuration was going on, lasted three days, after which the patient was infinitely better. The only thing of which he complained was a sense of fluctuation within the chest whenever he moved, and a feeling of oppression when seated. He laid equally well on both sides and had no other sign of an effusion than this fluctuation which he alone perceived. . . . . Ledran, thinking the case equivocal, took the advice of several colleagues, a majority of whom decided to wait for something more certain before operating. The fever continued and was accompanied with cold sweats, and the patient died on the eighth day. On opening the body, there were found about five pints of pus effused in the chest." (Boyer, Traité des Maladies Chirurgicales, vol vii, p. 362). If auscultation had been practiced in this case, together with percussion of the chest, I ask, would the

uncertainty of Ledran and his colleagues have been possible? "I am persuaded," says Laënnec, "that the operation for empyema will become more common and more useful, in proportion as mediate auscultation shall come into general use. In fact, up to this time, simple empyema and idiopathic hydrothorax, have scarcely ever been recognised, save in cases where the disease was of long standing, or of an aggravated character; yet many cases presenting even these conditions escape the attention of the most skilful surgeons and physicians, and for a much greater reason do the less severe cases which would offer more hope of saving the patient." (Traité de l' Auscultation médiate, vol. ii, p. 219).

5th. Another improvement to be added to those which auscultation has made in the art of diagnosis is this: henceforth to the attentive physician, there will be no more of those latent pleurisies, of which the ancients have reported so many cases, and which for the most part they appear disposed to regard as the result of the purulent melting of the lung. The stethoscope will always disclose the commencement and progress of the effusion, at the same time that it will furnish no less certain information relative to the organic lesion of the lungs, the symptoms of which predominate and mask those of the pleurisy.

6th. Lastly, with an attentive observer, there cannot be a development of pneumonia, the symptoms of which remain unknown, any more than there can be a latent pleurisy. So soon as the pulmonary substance becomes engorged, crepitation is developed; and it is by means of this sign that M. Piorry has been enabled to ascertain that the sanguineous and serous engorgement, almost always met with in the posterior part of the lungs of dead bodies, and which was regarded as a cadaveric phenomenon, a mechanical result of the gravitation of the fluids, often precedes death a good while, and constitutes a real disease mistaken for bronchitis; and that to this disease, which he has described under the name of *hypostatic pneumonia*, is to be ascribed the death of a great number of patients. We must not be deceived, and attribute to this phe-

nomenon of the latter periods of life, a more important part than it really plays. It is certain that when death approaches, as the consequence of any disease whatever, in proportion as the force of the vital principle relaxes, the organs previously sound no longer oppose a sufficient resistance to the causes of destruction, against which they are in an incessant struggle during life; and a species of vital decomposition precedes by some days the fatal moment when the body shall be unreservedly subjected to the exclusive influence of the physical agents which surround it, and of the chemical elements which compose it. Nevertheless it still follows from the interesting researches of M. Piorry, that there is often an inflammatory reaction in the dependent portions of the lung, whenever the weight or some mechanical impediment causes the fluids to remain there. Thence the indication to prevent the patient from remaining constantly on his back, and to make him lie alternately first on one side, and then on the other; and every time that the nature of the disease does not oppose it, to make him get up a while, and if he is able, walk about.

CHAPTER II.—OF PULMONARY CATARRH AND PHTHISIS.

I purposely include these two diseases in the same chapter, for before Laennec their confusion was almost inevitable. Indeed, if we consider these two affections in their extreme and most distant points, there is a very wide difference between them; and between a slight cold of a few days and a phthisis arrived at that state called by Bayle the third stage, error is impossible, and there is no need of the stethoscope to guard against it. But in certain chronic catarrhs accompanied by an abundant expectoration, and which last sometimes for whole years, how difficult it would be, without the stethoscope, to decide whether or not there be phthisis.

Cullen, endeavoring in his *Practical Medicine*, under the article *pulmonary phthisis* (vol. ii, p. 159), to establish a differential diagnosis between this disease and catarrh, says: "Catarrh or the expectoration of mucus is often accompanied by fe-

ver; but this fever is never, so far as I have been able to observe, like that which I am about to describe as hectic fever. This latter is, in my opinion, the most certain mark of a purulent state of some part of the body." He then describes hectic fever, to which he assigns as an essential character, its daily recurrence, chiefly at noon and in the evening.—There is a remission in the morning, but rarely apyrexia. It is accompanied with night sweats, and the urine deposits a furfuraceous brick-colored sediment. But similar fevers are sometimes observed in chronic catarrhs, which gradually undermine the constitution and sooner or later cause death, without there being the slightest ulceration, or the least tubercle in the pulmonary parenchyma. This is frequently observed in old persons, or in young children who die of pulmonary catarrh, following hooping cough.

Besides, we find this character of hectic fever indicated by almost all authors as the specific character of phthisis. Sauvages, who places this disease in the class *cachexiæ* and order *emaciations*, has thereby shown what idea he had of it. Shall I speak of the distinction that he makes between *tabid* fever, the character of which chiefly consists in emaciation, and *hectic* fever which takes its character from the intensity of the fever? This is too subtle a distinction. So with the twenty different species of phthisis which he describes, or rather points out; for in following the indications furnished by him it would be difficult to establish a certain diagnosis: and as to the differential diagnosis between phthisis and pulmonary catarrh, we ought still less to ask it of him; for it is pathological anatomy alone which has pointed out the material differences which separate these two diseases, and nowhere in Sauvages is pathological anatomy referred to.

We should not, however, believe that pathological anatomy alone, however well studied, could have brought about that immense progress which the stethoscope has made in the art of diagnosis of the diseases of the chest. What author has been more engaged in pathological anatomy than Portal, and yet what work is more barren of facts calculated to advance

the science than his, entitled: *Observations sur la nature et le traitement de la phthisie pulmonaire?* This work is composed of about one hundred and eleven observations on pulmonary phthisis, of which I find forty that terminated favorably. This result alone, already suffices to excite distrust in one who knows that, unfortunately, the most enlightened therapeutics have not yet been able to cure forty out of one hundred and eleven phthisical patients. We soon have the key to his success, when we see the author exclaim; "How many simple colds run into pneumonia or pulmonary phthisis in consequence of a stimulating treatment!" (Op. cit. vol. i, p. 369). In general Portal gives the name of phthisical to every patient, coughing, spitting, wasting and having a hectic fever. I have already said that we frequently find persons having only chronic pulmonary catarrhs, who likewise present a fever with emaciation, and yet the anatomical difference between catarrh and phthisis is so important that we may, without asking too much, demand of the authors who have written on these diseases, symptoms which distinguish the one from the other. In catarrh there is only a hypersecretion of mucus in the inflamed bronchiæ, in phthisis there is an organic alteration of the lung.

Although having the scalpel constantly in his hand, and terminating all his cases of phthisis by the autopsy of the subject, it would seem that Portal had no precise idea of the nature of the organic alteration which essentially constitutes pulmonary phthisis. Scarcely anywhere does he speak of tubercles, but of grumous concretions, of abscesses scattered in different parts of the lung. In the case of the abbé de Lamothe (op. cit. vol. i, p. 175), he gives as an example of phthisis from plethora, the history of a true pleuro-pneumonia, which the details of the autopsy do not permit us to mistake. But what still more confirms the judgement which I have passed on this author, is the fact that he establishes as many species of phthisis as there are causes capable of producing this degeneration of the pulmonary parenchyma. He describes *scrofulous, hereditary and plethoric* phthisis; phthisis *the consequence*

of inflammatory diseases of the lung (under this name we find described true chronic pleurisies); and phthisis following *exanthematous, catarrhal, rheumatic, scorbutic, calculous and venereal fevers.* Certainly the study of causes is not to be despised, any more than that of general and constitutional diseases with which phthisis may be complicated; but is it a sufficient reason for establishing all these different species, which, at the bottom, differ in nothing from each other?

However, I will not leave the work of Portal without awarding him due praise for having recognised the true nature of vomicæ, those abscesses which have attracted so much attention from nosologists. Portal regards them as being produced by an agglomeration of tubercles: "Vomica is on a large scale," says he, "what tubercle is on a small one." (Op. cit., vol. ii, p. 211). We shall see that this idea has been fully confirmed by the researches of Laennec.

Thomas Reid, author of an English treatise on pulmonary phthisis, does not better distinguish this disease from simple catarrh; the very definition which he gives, proves that he had not a very clear idea of its nature. "Pulmonary phthisis, when fully confirmed," he says, "may be defined, the expectoration of a purulent matter from the lungs, by the repeated efforts of a painful cough, accompanied by a fever of a peculiar kind, which produces morning sweats, suffers remission in the afternoon, and soon causes a considerable loss of strength and flesh." (*Essay on the nature and treatment of pulmonary phthisis*, translated by Dumas, p. 4). We find nothing in this definition that specially characterises phthisis, nothing which stamps it with a particular seal, distinguishing it from other diseases with which the lung may be affected, and above all from chronic catarrh, with which it is most likely to be confounded. Are there not indeed catarrhs to which this definition, or rather description, would be perfectly applicable in all points, without excepting the fever and loss of strength and flesh? Not that we do not see in Reid a very manifest design to distinguish real and consequently incurable phthisis (for, according to him, there is no cure to be hoped for), from

the diseases which simulate it. "The numerous cures of confirmed phthisis which are cited," says he, "often deceive us, and gain credence by our mistaking mucus for pus." (Op. cit., p. 39). Wishing to prevent similar errors, he points out the characters by which pus is distinguished from mucus. The chief of these consists in the fact that pus mingled with water separates on standing, and is precipitated to the bottom of the vessel, whilst this does not happen with mucus. But this differential character, to which Reid attached so much importance, is altogether insufficient to distinguish pus from mucus. Besides what would it prove, even were it as infallible as it is unfounded? What more common than puriform sputa in pulmonary catarrh? "We should give but little confidence to the inspection of the sputa in pulmonary phthisis," says Laennec, "because even the most characteristic, such as those which are ash-colored, or puriform and vermicular, are frequently met with in chronic catarrhs; and, moreover, the expectoraration of consumptives is, excepting a thousandth part, nothing but the product of a pulmonary catarrh, which almost always accompanies the tubercular affection of the lungs. (Traité de l' Auscultation médiate, vol. i, p. 689.)

If the efforts which Reid has made to distinguish affections purely catarrhal from true pulmonary phthisis have not been crowned with success, we must still praise him for having a clear idea of the essential difference between the two diseases. Phthisis is always the result of tubercles in the lungs, a good description of which he borrows from Stark. (Reid, op. cit,, p. 46). The parenchymatous substance of the lungs compressed by these foreign bodies, and removed, so to speak, from the vital influence, thus becomes susceptible of being dissolved and converted into pus, which of itself, by its own solvent properties, leads to softening of the tubercles, and causes them to burst into the interior of the bronchial ramifications. (Reid, op. cit., p. 52). All these things are perfectly correct, and, in truth, little has been added to this theory. We shall not, perhaps, find the same correctness in Reid's explanation of the hectic fever, which always accompanies the destruction of the

lung, and which he states depends on the fact that this organ, no longer receiving the same quantity of air as in the normal state, does not exhale the same proportion of lymphatic fluid. The portion of this fluid thus retained in the body, combines with the superabundant phlogiston to produce the fever. (Reid, op. cit., p. 107.) It was really not worth his while to take so much pains in combatting the ancient notions which attributed the fever of phthisis to the absorption of pus from the lungs, and its mixture with the blood, in order to substitute for it a theory so poor, so improbable, and so inferior to that which he rejects. Be this as it may, Reid can be said to have advanced farther, relative to the point which we are discussing, that is, the distinction between phthisis and pulmonary catarrh, than his translator, Professor Dumas of Montpelier. The former has at least tried to establish a differential diagnosis, and the insufficiency of his means of investigation is the only cause of his failure. Dumas, on the contrary, in the preliminary discourse which he has prefixed to his translation of Reid, endeavoring to prove that many chronic diseases take their origin in the pituitous constitution of the humors, adds: "that catarrhal fevers have a close relation, a manifest filiation with the disease of the chest called pulmonary phthisis" (Reid, op. cit., *Disc. prelim.* of Dumas, p. 16); and further on: "not only does catarrh give rise to phthisis, but the catarrhal fevers have also a great affinity with pulmonary hectic fever" (p. 67). Not wishing, though, to judge the ideas of Professor Dumas from a few passages taken from his preliminary discourse, I leave this subject, and pass on to Baumes, another professor of the University of Montpelier, so fruitful in illustrious men.

Baumes, author of a treatise on phthisis pulmonalis, which in 1784 took the prize of the ancient Royal Society of Medicine, lays down ulceration of the lung as the essential character of pulmonary phthisis, in the same manner that ulceration of the liver, mesentery, or kidneys constitutes hepatic, mesenteric or renal phthisis. Thus, then, phthisis pulmonalis is, according to him, an emaciation caused by the purulent liquefaction of the lungs. Beaumes was fully aware how much this

definition restricted the cases of real phthisis, and appears to have seen that it was obnoxious on account of its narrowness; for he immediately adds: "But the non-ulcerative affections of these same organs which, by their influence on the animal economy, bring about the destruction of the machine and death, are doubtless worthy of a particular denomination: it is thus that non-suppurating tubercles should constitute a special disease, to be called *pulmonary hectisis* (*étisie pulmonaire*), as schirrous engorgement of the mesentery, of the liver, or of the spleen, constitutes mesenteric, hepatic or splenitic *hectisis* (*étisie*), etc. (Op. cit., vol. i, p. 8)". In thus creating two genera of diseases of the chest, both having for their essential character emaciation and wasting, Baumes was ignorant of what has since been so well established, first by Laennec and then by Andral, Louis, and other pathologists who have followed in the same path, that these two genera, so distinct in his work, are confounded together in nature, and that the one always precedes the other; for pulmonary ulceration is always the result of the existence of tubercles which, after having remained for a longer or shorter time stationary and in a dry state, soften, and dissolve to some extent in the pus formed in consequence of their presence in the pulmonary parenchyma, and, being eliminated with it, leave an ulceration in the place they occupied. What line of demarcation, then, can be drawn between these two states? By what differential signs will they be recognised? It is still by an attentive examination of the products of the expectoration, that Baumes expects to find the distinguishing marks: and like Reid he multiplies experiments in seeking to distinguish pus from simple mucus. I will not here detail all the minute processes which he employs to attain this end, I will not cite all the reagents to whose action he submits the products the nature of which he wishes to determine; they can be seen in his work (vol i, p. 66). Were these processes as infallible as they are unfounded, what would they prove, since simple catarrhs, without ulceration of the mucous membrane of the bronchiæ, are sometimes accompanied by sputa differing in nothing from those of confirmed phthisis?

Although not regarding tubercular phthisis, as a real phthisis, unless when the tubercles have passed to the stage of softening, Baumes could not do otherwise than indicate the signs of the presence of dry tubercles in the lungs. He does so, after having described the general characters of the temperament which, according to him, predisposes to tubercles, and which is only the scrofulous temperament exaggerated. The pathognomonic signs which he indicates are: embarrassed respiration, which would induce the belief of asthma; a continual dry cough, increasing in frequency and intensity, in proportion as the tubercles become more numerous; difficulty of making full inspirations, which are followed by cough; short breath from the slightest fatigue; impossibility of singing; febrile pulse; emaciation; irregular sweats; general pallor, the cheeks alone remaining colored; suppression of the catamenia in women. These symptoms are correct, but at the present day they only warrant us in suspecting tubercles, auscultation, combined with percussion, being alone capable of rendering their presence certain. But in Baumes' view, this group of symptoms characterises only the predisposition to phthisis; and he was so completely ignorant of the inevitable necessity of the softening of tubercles, that he afterwards devotes some lengthy pages to the study of the proper means for encouraging a resolution of them, and for preventing hectic fever, without which he cannot conceive of phthisis. We shall again see that to have proved that tubercles once formed never retrograde, and can only be eliminated after having gone through the periods of softening and suppuration, is another of the triumphs due to auscultation. On the subject of vomicæ, how much less advanced is Baumes than Portal, who, however, wrote long before him! We have seen the latter acknowledge the identical nature of vomica and tubercle, the only difference being in their volume. According to Baumes, a vomica may be of two different species: inflammatory, or lymphatic. In both cases, it has nothing in common with phthisis; and the reason that many authors think they have cured consumption,

is because they have treated patients having vomicæ only. (Baumes, op. cit., vol. i, p. 408).

But if so many authors have been deceived, error must indeed be easy; and then, how is it to be avoided? This is what Baumes does not tell us.

Passing finally to the description of unequivocal ulcerated phthisis, Baumes indicates the following as the principal symptoms: hæmoptysis in the beginning, pains in the chest, sore throat and frequent hoarseness; a cough, somewhat resembling hiccup, and frequently causing only a sudden shock immediately before expectoration; change in the color of the face and flushing of the cheeks; frequent vomiting, by no means in proportion to the violence of the cough or the quantity of ingested aliments; night sweats, seldom involving the whole surface. Lastly come the signs drawn from an examination of the sputa, and from the presence of hectic fever. (Baumes, op. cit., vol. i, p. 59). In this description of open phthisis, we recognise the judicious and enlightened observer, who suffers nothing to escape him, and knows how to appreciate the importance of the most minute detail. Unquestionably this picture has an air of truth that would not allow of any indecision; but how many cases of manifest phthisis in which we find no such assemblage of symptoms! For example, how many patients are there whose lungs contain even considerable tubercular caverns, without hectic fever!

I find but little to notice in Bayle, although he has published a volume on the subject under consideration, (Recherches sur la phthisie pulmonaire, 1 vol.), in which there are some excellent anatomical details in reference to this disease. To this author is due the division, so long admitted, of the symptoms of phthisis into four stages or periods: *incubation*, *increase* (*début*), *confirmation* and *suppuration;* but he does not indicate the precise limits of each of them, nor any method of distinguishing the passage of one into the other, still less how to ascertain whether there is simple catarrh, or organic alteration of the lung: and, in fact, he has not been able to solve this problem better than the other authors whom I have

cited, on account of the insufficiency of the signs upon which he was obliged to found a diagnosis.

But suppose it were otherwise, that these diagnostic signs were of sufficient value and importance to enable us to affirm positively, whether or not there is organic alteration of the lungs, how could the unsound lung be designated in a case where one of them remained healthy? I very well know that in certain cases the thoracic pains of consumptive patients have such a fixed character, as to authorise the belief that the seat of pain is at the same time that of the disorganization. But with how many patients are these pains vague, often changing place, or if stationary, fixed between the shoulders, and hence attributed only to the fatigue and irritation of the bronchiæ, resulting from a continuance of the cough! I know well, also, that a knowledge of the precise seat of alteration is not immediately important, and will not modify, in the majority of cases, the therapeutic indications; but it matters not, this is a precision of diagnosis to which the use of the stethoscope has so much accustomed us, that at the present day an approximate (*à peu près*) diagnosis is not tolerated.

The method of Avenbrugger, suitably applied, enabled us to approach a little nearer this *localizing* diagnosis; but its author does not appear to have tried it in pulmonary phthisis. He speaks, however, of scirrhus of the lung and of vomicæ. Now, under these two names, we should understand infiltration of the parenchyma by tubercular matter, and then an abscess or cavity resulting from the sudden softening of the latter: for. as to real scirrhus of the lung, and degeneration of its substance into an evidently cancerous tissue, it is a rare thing, and probably would cause death even before the cancer could make much progress. Now, Avenbrugger indicates as a sign of scirrhus, the diminution or even suppression of all sonorousness in the affected part of the chest, at the same time that infrequent cough manifests itself, without expectoration, or attended only with viscid sputa, etc. (*Nouvelle Méthode pour reconnaître les maladies internes de la poitrine, par la percussion de cette cavité.* Translated by Corvisart, p. 275). Then

describing the signs that denote the softening of the scirrhus, and that the pus by its accumulation is about to form a vomica, he points out this very important characteristic. "If you place the palm of your hand on the spot where the vomica has been discovered by percussion, whilst the patient expectorates, you will perfectly distinguish the noise of pus in the interior of the chest, and so while the patient coughs. . . . . At this period, before the commencement of expectoration, the seat of the vomica recognised by percussion, yields exactly the sound of flesh when struck. But it yields an obtuse resonance as soon as the accumulated matter of a recently opened vomica is eliminated by the aid of a violent cough." (Ibid, p. 334).

The importance of all the physical signs mentioned here by Avenbrugger, it is needless to insist upon; their simple enunciation is enough to make us comprehend how much they have added to the precision of diagnosis. Suffice it to say that Laennec has in no wise changed them, but that auscultation has given them one degree more of certainty, by confirming them, or rectifying them when there was occasion, or lastly, by supplying their place in certain cases where it was impossible to obtain them. But I hasten to explain the changes which this method has brought about in the diagnosis of the two diseases in question.

In the first place, so soon as it has been practised with care, a differential diagnosis is at once indicated, although the labors of so many pathologists anterior to Laennec had not succeeded in establishing it; and this great result, the consequences of which are so important, is entirely due to auscultation. In pulmonary catarrh, the sonorousness of the chest being perfectly natural, we hear, in the part corresponding to the inflamed bronchia, a râle which varies in kind according to the nature of the catarrh; ordinarily sonorous, grave, sometimes sibilant, or else mucous when the expectoration is abundant. Besides, there is commonly an absence of the respiratory murmur in the part of the lung surrounding the diseased bronchia, a natural consequence of the obstruction of the latter by the mucus with which it is engorged. But this absence

of the respiratory murmur could never be attributed to a pleuritic effusion, for its seat is too circumscribed for that; neither to a pneumonia, nor to tubercles still in the crude state, because it is not permanent, but may cease and reappear suddenly: a fit of coughing being sufficient to bring on expectoration of the matter which engorges the bronchia, thereby restoring the respiration to its primitive state, until a new engorgement causes it again to disappear. Moreover, in the three diseases just named, percussion of the chest produces flatness, whilst pulmonary catarrh is always attended with very great sonorousness. And it is thus that these two methods, combined together, afford a mutual support, and correcting each other, bring about results of almost absolute certainty.

If the pathognomonic signs of pulmonary catarrh are so simple and easily recognised, those of phthisis are not less distinct.

Tubercles first accumulate at the summit of the lung, and it is almost always below the clavicle that the first signs of them are discovered. It is to auscultation that we owe the knowledge of this fact. Not that, while yet very small, and separated from each other by a healthy parenchyma, they are revealed by the least physical sign; the health is still good, and if the patient has a cough, it is of so trifling a nature that he rarely thinks of consulting a physician. But when once these abnormal productions begin to accumulate, the sonorousness of the chest diminishes in exact proportion to their size and number; and if both lungs are affected, as they seldom are to the same extent, percussion beneath the clavicles will produce an inequality of resonance which will indicate the more diseased lung. In this way the right lung is known to be generally more often affected than the left, or at least to be almost always the first. At the same time and in the same points, the respiratory murmur ceases to be heard, and is replaced by a diffuse and more or less marked bronchophony. When the tubercles begin to soften, the cough is accompanied by a characteristic gurgling, and the respiration, becoming cavernous,

denotes that an excavation is forming in the parenchyma. Bronchophony then gives place to pectoriloquy, which at first diffuse and obscure, becomes more and more clear and distinct. The dulness of the chest then disappears and is succeeded by a resonance which, at first might create the belief of an amendment, if the state of the patient, constantly growing worse, did not prove it to be apparent only. Pectoriloquy and cavernous respiration, which are true pathognomonic signs of pulmonary caverns, facilitate diagnosis to such a degree that not only the situation of the cavern, but likewise its dimensions can be exactly determined. These results have been attained by all persons a little practised in the art of auscultation. But if the cavity be superficial and very near the surface of the lungs, so that a thin layer of parenchyma separates it from the cavity of the pleura, percussion at the point where it is most superficial produces the sound of a cracked pot (*bruit de pot fêlé*), which is quite characteristic, and leaves no doubt concerning the cause that gives rise to it; and if a superficial excavation has a part of its thinnest wall not adherent to the pleura, it will be recognized by the muffled blowing sound (*souffle voilé*) which then accompanies the cavernous respiration and pectoriloquy.

Such was the importance of pectoriloquy, as a pathognomonic sign of the formation of a cavity in the pulmonary parenchyma following the softening of tubercles, that it was of the utmost consequence to determine its precise characters, and point out the causes capable of misleading the practitioner in the conclusions which he should draw from it. Valuable details on this subject may be found in Laennec, which I do not think it necessary to copy, as they are foreign to the question which I am treating; I think it right, however, to mention this fact, that the most evident pectoriloquy sometimes ceases to be heard for a longer or shorter time: a little mucus accumulated in the cavity which is its seat, or obstructing the bronchiæ which open into it, is sufficient for this. It is evident that in this case, the elimination of the matter by expectoration, is indispensable to the reestablishment of pectoriloquy.

In the appreciation of this phenomenon, there are some sources of error to be guarded against. Pectoriloquy being always produced by the resonance of the voice in the interior of an unnatural cavity, formed in the pulmonary parenchyma, the dilatation of the bronchiæ, which is sometimes equal to the size of the little finger when an entire bronchial ramification is dilated, or the volume of a filbert or even of a small walnut, when a part only of the bronchia is affected, must likewise produce pectoriloquy; and thus a mistake is easily made. M. Andral cites a case in which it was committed. (*Cliniq. Médic.*, vol. i, p. 187.) The 11th observation of M. Louis in his *Recherches anatomico-pathologiques sur la phthisie*, p. 235, is another example. But with some attention this error may in general be avoided. Dilatation of the bronchiæ, an ordinary result of chronic catarrh, is never preceded by hæmoptysis, is not accompanied by pains in the chest and symptoms of hectic fever, which are the ordinary concomitants of tubercular excavations. Moreover, in dilatation of the bronchiæ, pectoriloquy is never so distinct; it is generally heard over a space much too extensive to be attributable to a cavern, and lastly, it is almost always combined with sonorousness of the chest, which is scarcely compatible with the presence of tubercles in the lungs.

In general, pectoriloquy is the more clear and distinct as the cavern has denser and thinner walls, and as it is nearer the parietes of the chest: a cavern of middle size will afford a more evident pectoriloquy than a very small one. However, the phenomenon is sometimes very imperfect in large excavations. In those, for example, which attain a capacity equal to the volume of the fist, especially if there is a communication with some important bronchial branch, the pectoriloquy sometimes ceases altogether. The same is true in cases where rupture of the pulmonary parenchyma makes a communication between the cavity of the pleura and the excavation; but then pectoriloquy is replaced by two other phenomena equally certain: *amphoric respiration* and *metallic tinkling*. The amphoric respiration is rarely wanting, and much less frequently than

the metallic tinkling, which requires for its production, that there should be but little liquid in the cavern and that this should in addition be filled with air communicating with the bronchia. I have said that pectoriloquy also ceases, when a tubercular excavation bursts and empties itself into the pleura: the symptoms of pneumo-thorax, complicated with pleuritic effusion, are then developed. M. Louis has remarked that this rupture is ordinarily accompanied by a feeling of acute and sudden pain, attended with dyspnœa, and followed by the development of the symptoms of acute pleurisy. (Op. cit., p. 475).

Such are the new signs which Laennec has given to facilitate the diagnosis of a disease so important and so grave as pulmonary phthisis. I might say, too, that to him belongs the honor of having demonstrated beyond controversy that vomicæ, those abscesses of the lung which gave rise to such diversity of opinion among the ancients, are but the result of the sudden softening and instantaneous expectoration of a mass of tubercles. We have already seen that Portal held the same opinion. Laennec, by confirming it, has rendered a true service to science. But as it was through his anatomical researches, and not by means of auscultation, that he arrived at this result, I ought not to dwell longer upon it.

Let us now see what improvement they have effected in nosology and therapeutics.

1st. A line of demarcation has been for ever drawn between pulmonary catarrh and phthisis. Differing from each other in their seat, which is the mucous membrane of the bronchiæ in the one case, and the cellular tissue of the parenchyma in the other; in their nature, which is an inflammation in the first, and in the second an accidental inorganic production, with which inflammation has nothing to do; in their prognosis, which is infinitely less grave in catarrh than in phthisis, they have been shown to differ further in their signs, which render it impossible that they should be confounded. Now, to be able to distinguish a phthisical patient from one who is not, a case in which the prognosis must be extremely unfavorable

from one that offers many chances of cure, is felt to be important. The importance of this distinction would be greatly diminished, if phthisis pulmonalis could be engendered by degenerated pulmonary catarrh; but this opinion, generally received before the commencement of this century, has been shown to be entirely false. Although the wisely directed researches of pathological anatomy have contributed much to this result, auscultation has not been without its influence, and this is a second advantage to be noted.

2d. Indeed, carefully performed, and applied to a great number of patients, auscultation has shown that pulmonary catarrh does not contribute to the production of phthisis more than pneumonia itself. Persons laboring under pulmonary catarrh of many years standing, on being submitted to auscultation, have presented none of the signs which indicate the formation of tubercles; percussion of the chest eliciting a sound as sonorous as in the normal state, and the autopsy always confirming the diagnosis, and proving that pulmonary catarrh may extend its ravages even to ulceration of the bronchiæ, without a single tubercle being formed in the parenchyma. If phthisis is always ushered in by a cold, this is symptomatic only, and by no means the occasional cause of the tubercular eruption. What a difference there is, indeed, between the dry, hacking cough of phthisis at its commencement, and that of catarrh which soon takes on a humid character, in proportion as the expectoration becomes more abundant! Besides, nothing is more frequent than to find crude tubercles in the lungs of subjects that died of another disease, and had never had the slightest catarrh, and in whom these tubercles, still in a nascent state, had not been revealed by any symptom. I might add, indeed, that it is impossible to attribute tubercles to an accumulation of concrete pus, and that it is extremely probable that the physiological and morbid phenomena which we call *inflammation,* are not at all concerned in their production. But this would be entering into theoretical discussions foreign to my subject, and I forbear.

3d. One of the most interesting results which auscultation

has afforded, is the constant existence of a pulmonary catarrh, latent or manifest, during the whole course of continued fevers. At the onset and often during the whole course of the fever, it is accompanied neither by cough nor expectoration, and would be completely unsuspected, if the expectoration did not give rise to a sonorous rhonchus, sometimes very loud. M. Louis even says, in his *Recherches sur la fièvre typhoïde* (vol. ii, p. 238), that this râle is ordinarily much louder, and occupies a much more extensive space than the ordinary acute pulmonary catarrh, and that such is its disproportion to the cough and dyspnœa, that it constitutes a special and characteristic mark for typhoid fever: "so that its presence in doubtful cases where the affection is slight and the cerebral symptoms little marked, might render the diagnosis clear." Be this as it may, Laennec, who first pointed out this important fact, adds: "That this catarrh is sometimes unmasked at the approach of the crises. It constitutes the crises by sputa, noticed by the ancient practitioners, and which I myself have frequently observed." (Auscult. méd., vol. i, p. 195).

4th. In children attacked with hooping-cough, auscultation gives all the signs which indicate the existence of a pulmonary catarrh, namely: a respiratory murmur weaker or even absent in some points, with good resonance on percussion, a puerile respiration in other points, and sometimes a mucous, sonorous or sibilant rattle. But all these signs disappear during the fits of coughing: so long as these last, nothing can be perceived but the vibration imparted to the trunk; the wheezing and prolonged inspiration, which then makes the pathognomonic character of hooping-cough, appears to take place entirely in the larynx and trachea. These results, agreeing pretty well with those furnished by pathological anatomy, leave it beyond a doubt that in hooping-cough there is sanguineous congestion of the bronchial mucous membrane. But is this sanguineous congestion the cause or effect of the disease? Although the expression of my opinion upon this point, is quite useless in the question which I am treating, since the reasons by which it is supported are not at all founded on the results of auscul-

tation, I cannot forbear making it known. I will frankly state, then, that although auscultation combined with pathological anatomy has shown in hooping-cough the existence of an acute pulmonary catarrh, I can only see there a simple coincidence between bronchitis and the neurosis which constitutes this disease; a coincidence which is scarcely ever manifest, because the symptoms of the latter conceal those of the former. But sometimes the symptoms of each may show themselves separately, as M. Blache cites an example that occurred in one of his own children. In this young patient he observed a simple catarrhal cough, alternating with convulsive fits of the same. (Blache, art. Coqueluche, in the Dict. de Médicine, 2d edition). How, indeed, would a phlegmasia of the bronchiæ explain the peculiar and altogether abnormal course of hooping-cough; the absence of fever; the ease with which the spells of coughing return under the influence of moral causes, such as a fright or contradiction; the almost immediate return of the free exercise of the functions in the interval of the fits; the extraordinary obstinacy of this affection, far more difficult of cure than a bronchitis, etc., etc.?

In conclusion, I ought to say that Laennec, at the time of pointing out the existence of the symptoms of pulmonary catarrh, in hooping-cough, did not consider this disease as a simple phlegmasia: he describes it in his work under the name of convulsive catarrh, and openly proclaims its eminently nervous nature. If some physicians have fallen into the error of regarding hooping-cough as an ordinary inflammation only, it is not auscultation which has led them into it; but it is merely by judging the question with a determination to make this morbid affection agree with that exclusive theory, which considers inflammation the sole source of our diseases.

5th. Auscultation practised with care, and above all combined with percussion, will enable us to recognise in tubercles, existing as yet in the latent state in the pulmonary parenchyma, the source of the general symptoms, such as fever, emaciation, anorexia, etc., which otherwise could not be accounted for. Nothing, indeed, is more common than latent

phthisis; it is even probable that pulmonary tubercles do not manifest their presence by cough and dyspnœa, until some time after their appearance. But one thing, altogether inexplicable, and of which a doubt might exist if auscultation did not furnish the proof, is, that sometimes they commence by exciting general symptoms, before any local symptom reveals their existence. Hence the precept for every prudent practitioner to auscult all his patients, or at least those who afford the slightest foundation for a suspicion of tubercles.

6th. To auscultation, as much at least as to pathological anatomy, is due the knowledge of this fact, until then doubtful, the possibility of the cure of phthisis pulmonalis, at all events for a longer or shorter time. It is by the aid of this powerful means of investigation, that Laennec has ascertained that scarcely any phthisical patient dies with the first attack of this terrible disease; that the caverns excavated in the pulmonary parenchyma by the softening and evacuation of tubercles, persist in some instances for a great length of time, the patient preserving in other respects every appearance of health, and the autopsy subsequently showing them lined with a false membrane, which in time becomes almost cartilaginous, and forms a kind of wall protecting the parenchyma, which without it would have been exposed. But it must be granted, that this knowledge has not brought such satisfactory results to humanity as were of right expected. The certainty that now exists of the possibility of the cicatrisation of tubercular cavities, is well compensated for by another fact, not less absolute, that there is ordinarily and with almost all patients, a secondary irruption of tubercles, which will soften like the preceding and form new cavities in the parenchyma, will render hæmatosis still more difficult, and thus produce hectic fever and its terrible consequences.

7th. Thus far I have enumerated the advantages which have accrued to the diagnosis of phthisis pulmonalis and catarrh, from the discovery of Laennec: it still remains to be shown what are those which the therapeutics of these two diseases have received. It is evidently a great advantage, in the treat-

ment of pulmonary catarrh, to determine that the bronchial mucous membrane is alone affected. What confidence does it give the practitioner! How much more fearless must he be in employing methods of treatment, to the success of which he knows in advance that no serious organic alteration is opposed! In the case of phthisis perhaps a contrary effect has resulted, and it is difficult for the practitioner not to be discouraged when he has ascertained beyond all doubt that the patient who asks health at his hands, is attacked with pulmonary tubercles. But, in truth, can the reproach be cast upon auscultation? It is not the certainty of the evil which increases it, and it seems to me always better to know the gravity of the danger which the patient runs, than to remain in an uncertainty which is nowise advantageous to him.

Besides, auscultation and percussion combined (for the results, when separated, are as imperfect as they are useful when united), by enabling us to determine precisely the seat of pulmonary caverns, have furnished the idea of therapeutic means of which, without them, we should never have dreamed. It is thus that M. Piorry has tried to treat phthisis by compression of the chest, made on a level with the tubercular cavities, by means of a pyramid formed of compresses, and maintained by a roller. The results which he pretends to have obtained, are: the immediate suppression of the pectoriloquy, which is replaced by the natural respiratory murmur; a greater facility of expectoration, and at the same time a notable diminution of the matter expectorated. (See *Bulletin clinique* for June, 1836, and *Gazette Médicale*, 1836, p. 476). I have not heard that this method was generally attended with success; but it is an experiment which without auscultation would never have been made, and it is well that in such a disease as phthisis pulmonalis we can repeat this aphorism: *Melius est anceps remedium quàm nullum.*

But in auscultation as in all things human, it is difficult for the encomiums not to be tempered by some objections; and I think that as regards phthisis, some just ones can be made to this otherwise admirable discovery. Occur-

ring at a time when pathological anatomy, cultivated with more ardor than ever, was rapidly marching to its avowed end, of *localising* all diseases, and of striking general affections from nosological tables, auscultation, by enabling the progress of pulmonary disorganisation to be followed from to day, has still more favored this vicious tendency of the medical spirit of our time. Since its introduction, in the view of many physicians, phthisis has been a mere local disease, limited to the lung, and affecting the rest of the organism by sympathy only. How many powerful objections, however, are to be made to this theory! Why is the hectic fever not at all in proportion to the extent of the lesions presented by the lung? Why do we see consumption and marasmus in their last stage, accompanying small pulmonary cavities, in persons who yet have a very large portion of healthy parenchyma, whilst others, whose lungs are three fourths destroyed, still survive for a very considerable time? Why this constant coincidence of lesions of the larynx, of the stomach, and of the intestines, with destruction of the lungs by tubercles? Is it sympathy of the lung with those organs that causes it? But then, why does not acute pneumonia provoke these sympathies? Lastly, how can we explain the simultaneous developement of tubercles in almost all the organs; and this law, which, without being so constant as M. Louis thought when he promulgated it, is nevertheless often confirmed by experience: *that tubercles are never found in any organ, without they exist at the same time in the lungs?*

Phthisis pulmonalis is not then a local disease, but it affects at the same time the entire organism. It results from a morbid action which goes on in the whole body at the same time; a profound alteration of nutrition appears to be its cause, and the ancients were not wrong in describing a *tubercular constitution*. Lastly, men of superior minds have not failed to protest against this unhappy tendency, given to the study of phthisis by auscultation; and Dr. James Clark published at London, in 1834, a *Treatise on Consumption or Tubercular Phthisis*, whose object was to bring back practitioners to the study of the *tubercular constitution and cachexia* respecting

which he puts forth some eminently judicious opinions, but which it does not enter into my plan to develope here.

CHAPTER III.—OF PNEUMO-THORAX.

The present chapter will serve in some degree as an appendix to the two preceding, for the disease we are now considering is in fact almost always the result of a chronic pleurisy, or of phthisis pulmonalis. Under the name of pneumo-thorax, is designated an accumulation of gas or of elastic fluids in the thoracic cavities. Do the cavities of the pleura contain any in the natural state? the general opinion is that they do not. However, Laennec thought with M. Ribes that the pleuræ, in the healthy state, might contain a small quantity, arising from the vaporisation of the liquid which constantly lubricates their internal surface. Be this opinion as it may, it is certain that this gas, if it exists, is always inappreciable in autopsies. But it sometimes happens that the pleura is so filled that the accumulated gas forces the lung back towards its root, and distends the parietes of the thorax in a very perceptible manner.

This disease, although far from being rare, had not attracted the attention of any author, until M. Itard, now dead, physician to the deaf and dumb, chose it for his inaugural dissertation. But if the honor of its first description does not belong to Laennec, that of having first fixed the diagnosis in a certain manner, which we shall soon prove to be extremely difficult, if not impossible without auscultation, will not be denied him.

A gas may be found in the pleura, even in great quantity, without there being the slightest solution of continuity, the least alteration of this membrane, or any effusion whatever into its cavity. But this is not the most common case. Nearly always there is at the same time an effusion of liquid into the pleura; whether this liquid be pus, as it most frequently is, or serum, as in hydro-thorax, or blood coming from a penetrating wound of the chest, or lastly, tubercular matter from a vomica opening into the pleura; in all these cases, save the last, we are forced to admit, that the gas contained in the pleura is the

product of the putrid decomposition of the effused liquids, an origin which the fetidity of its odor often induces us to suspect. When the effusion consists of tubercular matter from a cavity, if the latter does not communicate with the bronchiæ, we are yet forced to attribute the production of the gas to the decomposition of the effused matter. But if, which is more common, we find in this cavity the orifices of several bronchial ramifications, there is no longer a doubt that the gas is nothing more than atmospheric air which has passed into the pleura.

We have already seen that this affection had been almost forgotten by the majority of authors anterior to Laennec; regarding it only as a rare phenomenon, of little importance, no endeavors were made to determine its peculiar symptoms. Had this been attempted, however, success would have been impossible. How, indeed, could general symptoms, so indefinite as those drawn from the state of the pulse, from the inspection of the tongue and of the excretions, have served for the recognition and discrimination of the numerous diseases with which the chest may be attacked? In this respect, therefore, the method proposed by Avenbrugger, and so ably developed and applied by Corvisart, made an immense step in pathology, and opened the way for the improvement which auscultation was to effect in the same science.

Percussion alone, however, could not in all cases distinguish hydro-thorax in a precise and indubitable manner. Where this affection exists in a high degree, the progressive augmentation of the clearness of the sound in the side primarily affected, may indeed cause it to be suspected; but a source of grievous error has to be guarded against. The greater dulness on the healthy side may make that be regarded as unsound which is in a normal state, and as healthy, that precisely which is the seat of disease. Still, this mistake could not be committed, save by one who had not followed the disease in its whole course, and besides, the pain existing in the side affected with pneumo-thorax would in most cases prevent it.

M. Piorry, in the article relative to this disease, in the *Dictionnaire des Sciences Médicales*, has proposed, as a very ex-

cellent means of detecting the complication of pneumo-thorax with an effusion in the pleura, to percuss the patient in different positions. The liquid, being the heavier, makes its way to the lowest part, and the sound is successively clear and flat in the same portion of the chest. But the application of this method is difficult in the majority of cases; and to obtain the most conclusive results possible, it would be necessary to percuss the patient first in the vertical position and then in the reverse situation, in order that the summit of the chest should for a moment become the base, which is morally impossible. Lastly, if there existed adhesions, percussion might lead into error. This is true, but it should be added, that these adhesions would become sources of error in all other methods, even in auscultation.

Ought we to rely more on abdominal pressure, proposed by Bichat and still lauded by M. Piorry, to aid the diagnosis of thoracic diseases? It is certain that, all things equal, abdominal pressure is incomparably more painful for patients suffering under an effusion of air or of water in the chest, than for those who have an inflamed lung only. But what vague information does such a means furnish! What uncertainty would a diagnosis have, founded on such a basis! Besides, abdominal pressure will be more or less easily practised, not only according to the greater or less vacuity of the chest, but also according to the greater or less sensibility of the patient. And finally, had this means all the efficacy that its author attributes to it, it would never serve to indicate the nature of the fluid which fills the pleura, and could not make known whether it was a gas or a liquid which prevented the diaphragm from being carried up, by abdominal pressure, as far as it would go in the natural state.

Auscultation alone, then, can furnish information sufficiently certain to enable us to recognise pneumo-thorax in the plurality of cases, if not in all: this will be easy for me to prove.

We must distinguish two species of this affection: 1. pure pneumo-thorax, that is, the simple accumulation of gas in one

of the cavities of the chest, without effusion in the pleura; 2. pneumo-thorax complicated with effusion.

Pneumo-thorax without effusion is extremely rare, but yet a few cases have been reported: (see Obs. xxxvi of Laennec, vol. ii, p. 249). It will be known by the affected side yielding a much clearer sound on percussion than the healthy one, by the complete absence of the respiratory murmur in the diseased side, the other continuing to present it as usual. Thus absence of the respiratory murmur coinciding with increased sonorousness of the chest, is the pathognomonic sign of simple pneumo-thorax. Pulmonary emphysema, it is true, presents analogous symptoms, and particularly the same coincidence; but in emphysema, the respiratory murmur is never so completely absent as in pneumo-thorax: and if it may sometimes appear so, it will then be wanting in the whole extent of the diseased side of the chest without exception; whilst in pneumo-thorax, however considerable may be the quantity of gas accumulated in the pleura, the respiratory murmur may always be heard along the vertebral column, a point where the compressed lung is found, precisely as we have seen it in hydro-thorax. Besides, pulmonary emphysema is developed slowly, and may exist in a high degree, without the patient ceasing to follow his occupation; whilst pneumo-thorax comes on suddenly, attains at once a considerable developement, and occurs only in bed-ridden patients already weakened by long suffering.

Pneumo-thorax with effusion, is, as we have said, the most frequent of all. When it exists, whether complicated or not with pulmonary fistula, and with a communication between the cavity of the pleura and the bronchiæ, it is the only case of effusion to which can be applied the exploratory method advised by Hippocrates for detecting empyema: I mean the succussion of the chest. It is certain that when pus is contained in the pleura without occupying its whole capacity, the rest being filled by an accumulation of gas or of atmospheric air, succussion of the chest will most often produce a sound easily recognised by the stethoscope, and which is sometimes

heard by those standing near. We may read in Laennec the learned dissertation which he has given on this method of the Asclepiades, which so long remained in utter neglect, because it was not understood by the various commentators of Hippocrates. Almost all the authors who have studied this passage, have regarded the method as impracticable, and above all as affording no result. This opinion, true as it respects ordinary effusions, is evidently false so soon as it refers to hydro-pneumothorax: and it is an established fact, that to hear the noise of fluctuation in the chest, is a proof that this cavity contains gas, besides the effused matter; for the liquid can not fluctuate except in a space filled with air.

But this sign, however important it may be, is fortunately not the only one by which the lesion we are studying may be recognised. Laennec has indicated two others, which, when they exist, place it beyond a doubt, that air and pus are simultaneously contained in one of the cavities of the pleura. These are, the *amphoric respiration* and the *metallic tinkling*. Laennec thought that metallic tinkling could take place only in cases of fistulous communication of the bronchiæ with the cavity of the effusion; but it appears, from some rare, though well observed cases, that this condition is not indispensable: it is so, however, for the amphoric respiration, whose presence denotes necessarily the communication of the bronchiæ with the pleura.

What is the cause of the metallic tinkling? Here we find a perfect confusion among authors, all of whom attribute it to a different source. Laennec thought it was owing to the fall of a drop of liquid from the upper wall of the chest back on the effusion below, when the patient rises suddenly in his bed. His opinion was founded on the circumstance that a real metallic tinkling is heard whenever a drop of water falls from a small height into a half-filled decanter. To him the similarity of the results proved the analogy of the causes.

M. Beau, in an article inserted in the *Archives générales de médicine* (March 1834), has proposed another theory for the metallic tinkling. This physician thinks that this really char-

acteristic noise is always produced by the rupture of an air bubble in the midst of a thoracic, pleural or cavernous effusion. According to him, it can be produced by this cause only; but as certain patients sometimes present this noise several days in succession and without interruption, it may be asked, what would become of the enormous quantity of gas which would always be accumulating in the pleura, if the proposition were true.

The opinion of Laennec, founded on fact, could evidently not be applied to all cases of metallic tinkling. That of M. Beau, true also in some circumstances, can no more pretend to the honor of explaining all the cases. Therefore many authors have admitted both, as accounting for the various circumstances which favor the production of this noise. But some have indicated other circumstances as indispensable to the occurrence of this phenomenon. Thus, Mr. Guthrie affirms that the presence of a certain quantity of compressed air is a *sine quâ non* for the production of the metallic sounds. Dr. Thomas Davies thinks that this air must be agitated at the surface of the effused liquid; and Dr. Houghton regards the sounds as a mere echo, which is itself produced by the fall of a liquid in the cavity, and the entrance of air through a fistulous opening.

Other authors have rejected all these theories, to substitute others which do not appear to me even so admissible. Among these I will cite only Dr. Bigelow, who, in his remarks on pneumo-thorax in the 4th number of *The American Journal of Medical Sciences*, for 1838, after having stated all the reasons which prevented him from admitting the theories of Laennec and others whom I have cited, lays down as principles, resulting from the experiments performed by himself: 1st. that "the immediate or exciting cause of metallic tinkling, is a sudden and violent commotion of the liquid in a vibrating cavity . . . . . , the same phenomenon taking place when a part of the liquid is thrown up by the cough, and then falls back on the rest:" (is this not a return to the falling drop of Laennec?); 2dly. "the amphoric resonance is produced by the reverbera-

tion of the air in a vibrating cavity, and without the sonorous impulsion of the liquid, etc."

However it may be with these various explanations, let us note as a fact of great importance that the value of the metallic tinkling has not changed, and that it is always the pathognomonic sign of an effusion of air and liquid into the same cavity, this cavity almost always communicating with the bronchiæ. I ought, however, to state, that the metallic tinkling and the amphoric respiration may also present themselves in the case of a very large, but nearly empty pulmonary cavity. But, even in this case, "it is difficult to be mistaken," says Laennec; "a remnant of pectoriloquy, the small extent of the space in which the metallic tinkling, the amphoric resonance, and the tympanitic sound given by percussion are heard, and the absence of fluctuation, characterise a vast pulmonary cavity; the cough, on the other hand, sometimes produces a gurgling or a slight fluctuation which it never does in pneumo-thorax." (Laennec, op. cit., vol. ii, p, 357).

After what I have just said of the new signs furnished by auscultation in the diagnosis of pneumo-thorax, it will not be difficult for me to tell how much this discovery has advanced science on this point: it has enabled us to detect during life an affection which formerly was never perceived before death, and even when recognised on the dead body, was considered as a cadaveric phenomenon only, an occurrence of the dying moments. Laennec says in his work, that whilst he was following the lectures of Corvisart he several times saw him make examinations of subjects affected with pneumo-thorax, in none of whom had this affection been suspected. "No one will deny," he adds, "to this celebrated professor either the talent for observation or the ability to take advantage of percussion; and consequently the best proof which can be given of the insufficiency of this method of recognising pneumo-thorax, is that the latter was not detected in these cases." (Laennec, op. cit., vol. ii, p. 259). Certainly an affection unperceived by Corvisart is of difficult diagnosis; and it has not been a small service rendered to science, to make this diagnosis easy to all.

If it should be asked what advantage has resulted from it to the therapeutics of this disease, I answer, that it is already a very great one to have fixed the nature of the lesion with which the patient who claims our aid is affected. Now, precision of diagnosis is necessarily an approximation to a more efficacious mode of treatment; and though we have not yet attained the desired end, it is nevertheless a great good to know the extent of the evil which we wish to avert. But, if we reflect that pneumo-thorax is most frequently a mode of termination of a disease generally fatal, either chronic pleurisy or phthisis pulmonalis; if we consider that it is most generally a phenomenon of the last stages of the disease, we will see that it was quite natural that so grave a case should present few therapeutic indications, and we shall be less disposed to render auscultation responsible for the little progress which science has made with respect to its treatment.

(TO BE CONTINUED.)

Bibliographical Notices.

Art. II.—*The New England Quarterly Journal of Medicine and Surgery.* Vol. 1st, No. 1, for July, 1842. Boston, pp. 156.

Cunard's steamers and the "Western Railway" seem to have given an impetus to more things than commerce in Boston: a new-born excitement has taken hold of the profession. Hitherto, notwithstanding her advantages for such an enterprise, the only medical periodical published there has been a small, neat-looking, "one-masted" vessel, (we might have said one *mastered*) yclept the *Boston Medical and Surgical Journal*, under the guidance of that learned Theban and experienced navigator, Jerome Van Crowninshield Smith, M. D., of *ichthyological* memory, who is as punctual in his visitations as a bank notice, for which he deserves and receives due thanks. But here we have a new craft looming up in the distance, a regular "three-decker," we suppose it might be called, though not a vessel of war, which is to anchor and lay its cargo at our door every three months.

To drop such an incongruous metaphor, though, ere it has led us into error, if it has not already done it, and speak in landsman's phrase, as best becomes one who never snuffed a sea-breeze, we would simply say, that the above is the title of a new Quarterly, whose first number has been lying on our table some days, having with a sort of rail-road celerity reached us considerably in advance of its appointed time. It is edited by Charles E. Ware, M. D., and Samuel Parkman, M. D., names not unfamiliar to the reader, and is *backed* by some of the best pens in New England.

The first feeling we had, when, two months or more since, we copied a notice heralding the advent of this journal was one of surprise that something of the kind had not been undertaken long before; and we must still express the same feeling. It would be superfluous to say that there is plenty of talent and material in Boston for such a work, when some of our best writers, both surgical and medical, are or have been residents of that city. And as to support, the territory comprehended under the term New England, ought by all means to sustain two journals, even supposing their circulation to be confined to its limits, which is not the case with the weekly already mentioned, and of course will not be with the new quarterly.

Some of our exchanges—the New York Lancet for example—predict that it will be a failure on account of its *trimestrial* character, and express a doubt whether any thing can succeed save a cheap weekly journal. With due deference to them, we think differently. The rage (if such there be) for hebdomadals, will be short-lived, and work its own cure, to which nothing will tend more promptly or effectually than their multiplication. We do not deny them a measure of utility; they quickly disseminate a knowledge of the *novelties* occurring in the profession, in which respect they are attractive and peradventure useful; but from their very constitution they can do little more. For solid learning, for extensive research, for all, in short, that can give character and dignity to the profession, that will set its members to thinking and reflecting, and result in permanent improvement, a monthly or quarterly will be required. There are few readers, we opine, who do not look with far more interest and anticipated benefit for the American Journal of Medical Sciences, or some of the foreign quarterlies republished in this country (modesty forbids the mention of our *monthly* in such a connection), than for any of the weekly periodicals now existing among us. Boston has quick communication with the world beyond the waters, she has numerous and large hospitals, extensive libraries, a corps of liberal and enlightened medical men—wherefore, then,

should she not possess a valuable and popular journal of the kind now offered to the public?

This number exhibits a handsome appearance—nothing flashy or dandyish—both inside and out. Its contents are of a diversified character, consisting of nine original papers on medical and surgical subjects, with reviews, bibliographical notices, scientific intelligence, and a variety of selected matter. The original papers are all of a valuable nature, such as will at once give a high standing to the work; one of them will be found in our pages for this month, and we shall take an early opportunity to introduce our readers to some others that we have marked for transfer. Under the head of scientific intelligence are extracts from reports made to the Boston Society for Medical Improvement, which contain much that is of interest; this is a feature that will be continued, we presume, in the future numbers. On the whole we augur very favorably of this periodical, and give it a cordial welcome. If any of our readers crave more food than is served up to them in our monthly dish, we commend the New England Quarterly to their favorable notice.

C.

Art. III.—*The American Journal and Library of Dental Science.* Vol. II, No. 4, June, 1842. Baltimore.

There was as much truth as wit in the wag who said he never saw "M. D." after a dentist's name, that he did not read it "*miserable dentist*"; as much truth, we say, as wit, as many an unfortunate young gentleman and gentle "spinster" can abundantly testify. Of all professions or callings, of all occupations or employments, of all sciences or arts that have ever risen and flourished, we venture to give it as our firm belief that there have been more absolute, unredeemed *bunglers* and *botches*, more downright, incorrigible *ignoramuses* and *dolts*, in that of dentistry than all others put together. We have seen and heard of so much irreparable injury inflicted by them, if not with malice prepense with at least something akin to it, that we freely confess the appellation is, or rather was, any thing

but a badge of knighthood, or a stamp of gentility in our view.

Such, we say, was the case; but it is a source of pleasure to think that the day has gone by when a fellow, too lazy to work, provided with a leaf or two of gold foil, or something resembling it, an old file and a pair of forceps, could set out, advertise himself "dentist," with a long string of recommendations, and commence his murderous work of rasping, filing and filling. Men of talents, and many of them possessing high attainments, have entered the field and wrested it from the occupancy of mountebanks and greedy charlatans, driving out these unclean spirits who infested it. Both as a science and an art dentistry has made rapid advances within the last few years, and bids fair soon to attain an honorable rank among the departments of surgery. To practice this profession successfully now, not only is great mechanical skill required, but likewise an intimate acquaintance with the laws and phenomena of diseased action as it affects the whole or a part of the body. A man can set him down now in a dentist's chair (what an inviting air they all have, too!), without fear of being mangled or crucified—of undergoing tortures to which the rack, iron boots, and screws of Torquemada, or the live coals of Cortez, would be beds of down, and exquisite pleasure.

In extending the reform thus happily commenced, the Journal will prove an efficient instrument in the hands of the association by whom it is published, and afford powerful aid to the Baltimore College of Dental Surgery recently established by the same Society. For this reason, therefore, if there were none other, it commends itself to the patronage of the profession. Its appearance, to use the language of a contemporary, "is every way creditable"; indeed, as to typography, it is one of the most neatly printed journals that we have seen. No pains or expense seems to be spared by the conductors to render it valuable. The journal department of the present number, which completes the second volume, contains several interesting papers, both original and selected, and is embellished with numerous wood-cuts. The third volume commences in September next, and we hope will be issued to an increased number of subscribers. C.

Selections from American and Foreign Journals.

Secondary hemorrhage after amputations—comparative advantages of circular and flap amputations.—In a discussion in the Royal Medico-Chirurgical Society, May 11, 1841, "Mr. Rutherford Alcock said that the question of amputations generally, and of the different modes of performing them, and of the effects of different circumstances upon the results of the cases, had engaged much of his attention for several years. Many of the results which he had attained were already before the public, and he would now limit himself to observations on the principal topics of the present paper, and especially to the question of the comparative advantages of the flap and circular modes of operation, and the results of each with reference to the ultimate terminations of the cases, and the several accidents that supervene on amputations. With respect to the occurrence of secondary hemorrhage, he quite agreed with Mr. Liston that the oblique division of the femoral artery was not a matter of importance. But he was not sure that the oblique division of the smaller arteries had not some influence in bringing on or permitting that accident. He had often thought that, in flap amputations, the small vessels retracted only partially, which, in circular amputations, would have retracted completely; or that the vessels in the former case were overlooked, which, in the latter, bleeding distinctly, would have been secured. Nothing, however, had convinced him more of the necessity of having accurate records of series of cases, and of not trusting to general impressions, than the results of amputations. In these questions, more than in any others, general impressions were apt to be erroneous, from the circumstance of the strength of the impression which single events, deemed at the time remarkable or important, make upon the mind. With respect to this very

question of secondary hemorrhage, for instance, he had himself gained a very wrong impression from two cases, which was not corrected until he had accurately ascertained the results of the whole series of those that had fallen under his observation. The two cases were these:—In an epidemic from which the troops under his charge suffered at Vittoria, the legs in several cases became gangrenous, and required amputation. In the first two instances he amputated in each patient one leg by the flap, and the other by the circular operation; and in both these cases the flap operation was followed by secondary hemorrhage; but after the circular none supervened. The circumstance made such an impression on him that he entertained no doubt that flap amputations were more obnoxious to secondary hemorrhage than circular, till he came to sum up the results of all his cases, or, at least, of all those of which he had a regular and well-kept series. The result of this examination was, that in 115 cases, of which 90 were circular amputations, and 25 flap amputations, the proportion of secondary hemorrhages was somewhat greater in the former than in the latter. In the second place, with regard to the liability to necrosis of the sawn bone in the two classes of cases, he did not think that any difference could be found between them; and it was the same with regard to the conical form of the stump, an event which he believed could only very rarely occur after either kind of amputation, if dexterously performed by a surgeon practised in the operation. As in the 115 cases alluded to it had occurred only twice, he could not draw any conclusion respecting its frequency in one or the other operation. With reference to the main question of the comparative mortalities of each mode of operation, he has found that his results were by a fraction disadvantageous to the flap amputations. He was not, therefore, prepared to agree in what was said to be the growing opinion that this operation would in time completely supersede the other. It was true that the flap amputation was a more rapid mode of operating than the circular, and to this full weight must be allowed; for pain is in itself an absorbent of life, and is always followed by a reaction and a febrile disturbance, that may have an important influence on the recovery of the patient. At the same time he thought that the difference in this respect between the two modes of operation had been overrated. Eighty or ninety seconds were ordinarily sufficient for the performance of the circular operation; and if the flap amputation were done in fifty or sixty, the difference of the times during which the patient was in each exposed to pain was really not so great that much

much importance could be attributed to it. It did not appear to him, therefore, that on this ground there could be any just reason for discarding the circular mode of operation. A more important point seemed to him to be involved in the question of the after treatment of the stump. There was nothing of which he was more certain than that the idea of the advantages of union by the first intention, which was so commonly held in this country, was greatly exaggerated, and he would go so far as to say that, in some cases, such a union was not only of no advantage, but was actually mischievous, and dangerous to the life of the patient. For patients, after immediate union of the wound, would often, as he had himself seen, die of the very diseases which were said to result from the wound remaining open. In a primary amputation, after an accident in a healthy man, it was no doubt most natural to desire that the wound should be healed as quickly as possible; and, probably, in these cases, it was best to try to obtain a union by the first intention. But in secondary amputations, and in those performed for diseases of the limbs, when the incisions were made through diseased tissues, there was no use whatever, but, he believed, a disadvantage, in bringing them together. They would almost invariably open again, and place themselves in the position in which they had been left from the first. This was especially the case when limbs were amputated for diseases which had been connected with a considerable discharge. In all these it was dangerous to stop the discharge too suddenly, and it had been proposed to make an issue near the end of the stump, to keep up an artificial drain to replace that which had been cut off. But this method, he thought, would be very inconvenient, and such indeed as could scarcely be put in practice; another, which he had sometimes adopted, and which seemed to fulfil all the intention, consisted in passing a skein of silk through the lower part of the wound, and thus maintaining, for a time, a constant irritation and a discharge of matter, which afterwards might be gradually reduced."—*Am. Jour. Med. Sci.*, from *London Med. Gaz.*, May, 1841.

Fracture of the tibia, with avulsion of the internal malleolus, and penetrating wound of the ankle-joint; consecutive abscesses; necrosis of the tibia, and resection of its inferior extremity—recovery. By M. Castella.—C. H. J. St. Croix, aged 42, was thrown down on the 22d of May, 1840, under a bag-

gage wagon. The right foot was turned forcibly outwards, the internal malleolus was torn from its attachment, and the inferior extremity of the tibia protruded through the skin to a considerable extent.

Considerable inflammation followed the injury, abscesses formed behind the tibia, and amputation being deemed necessary, the patient was conveyed to the hospital at Pourtales, on the 27th of May.

The right foot was found to be very much averted—the fibula was fractured three inches above the external malleolus, which was flattened and crepitated on pressure—and above which the fibula presented a reentrant obtuse angle. Internally the inferior extremity of the tibia, black and denuded, projected to the extent of two inches, and the internal malleolus was torn off at the level of the articular surface of the tibia. Several fistulous openings existed at the posterior border of the tibia, and yielded a fetid sanious discharge. The foot was œdematous; there was considerable fever, and the patient was very much debilitated.

June 2d.--Before resorting to amputation, it was determined to resect a portion of the necrosed tibia, to replace the foot in its natural position, and to retain it in situ by means of Dupuytren's apparatus for fracture of the fibula.

With this view, an incision was made anteriorly and posteriorly to the tibia, and a slip of lead passed round the external surface of the bone, as high up as possible, to protect the soft parts, and especially the vessels; the bone was then sawed across, and twenty-one lines of its inferior extremity removed.

The foot was now easily drawn inwards, and kept in position, with Dupuytren's bandage; the depression of the fibula disappeared; and the leg being semi-flexed, was placed on its outer surface.

Phlegmonous inflammation followed the operation, requiring several incisions which gave issue to sloughy cellular tissue; but at length granulations covered the inferior extremity of the tibia; the medullary membrane became rapidly developed, and in proportion as the vacant space between the tibia, fibula, and astragalus became filled with granulations, the heel which had been drawn forcibly upwards by the action of the gastrocnemii and solei muscles, resumed its situation, and the foot ceased to be extended on the leg.

At the expiration of two months the fibula was consolidated, and presented a symmetrical outline; and after a few months, the wound was completely consolidated. The foot had acquired some small degree of motion, and the space resulting from

the resection of the tibia had disappeared, and was filled by a solid bony substance. In October the patient began to walk on crutches; and on the 11th October he left the hospital, being able to walk with the aid of a stick.—*Dublin Med. Press*, from *Gazette Médicale de Paris.*

[This case resembles in some respects that of Wm. Smith, which was noticed in the February number of our Journal, p. 141. The mode of treatment adopted is likewise similar to that so successfully pursued in the latter by Dr. Shipman, of Cortlandville, N. Y. We may mention, by the way, that a correspondent writes us that Smith is entirely well, and has walked 30 miles in one day since last winter; his injured limb, save that it is shorter, being equally as serviceable as the other. Dr. Shipman's triumph over his persecutors, or rather we should say, the triumph of science and skill over ignorance and dulness, is therefore complete. Thus may it ever be. We commend the case above quoted to the especial notice of Professors Hamilton and Webster of the Geneva Medical College, and to all others who can do nothing without "authority from books."—Eds.]

Comparative frequency of tuberculous disease. By J. B. S. Jackson, M. D., of Boston, Mass. The following is an analysis of dissections made during the last ten years in this city or the immediate vicinity, and tending to confirm the general statement, that in the temperate latitudes about one sixth or more of our race die from some form of tuberculous disease.

The whole number of autopsies I find to be 604.

Of these there were excluded 94 cases of patients dying from disease foreign to the lungs, and in which these organs were not at all, or not sufficiently, examined; also four cases in which there was a question between pneumonia and tuberculous disease.

Of phthisis there were 93 cases. Amongst these were included some cases of general tuberculous disease in children, and in which some of the other organs may have been as much or more affected than the lungs. One of these last was a child that died of tuberculous disease of the brain, with considerable disease of the bronchial glands and only a trace in the lungs.

Acute meningitis in 16 cases, tuberculous disease being found in all of them, either in the lungs, the bronchial glands, or in both. The disease was undoubtedly tubercular, though in the first four cases reported, granulations are not mentioned as having been found in the membranes of the brain, not being acquainted at that time with the true nature of the disease.

A case of tubercular peritonitis, also, may be mentioned, in which, though the bronchial glands were considerably diseased, a single granulation only was found in the lungs.

In the above 604 cases, then, death is supposed to have been caused by tuberculous disease in 110, or in about one in 5½.

In the remaining 396 cases, death was caused by some other than tuberculous disease; but in all the lungs were examined, and in a large proportion it is expressly stated whether there was, or was not found any such disease. Without doubt, the existence of tuberculous disease was occasionally overlooked, but, on the contrary, it was often noted when found in a very small or even minute quantity.

In the above 396 cases, there was no tuberculous disease nor any remains of any found in 306; the wilted appearance so often met with at the apex of the lungs, not being considered as satisfactory evidence of the disease having formerly existed. The existence of cretaceous matter, however, was so considered, and cases where this was found were noted as tuberculous, without any discrimination. In 46 cases, tubercles were found in the lungs alone, and in 21 in the lungs and bronchial glands. In 20 they existed only in the bronchial glands, and were in the cretaceous form in all except two. In two cases there were cretaceous masses in the upper part of the thorax, supposed to be the result of old tuberculous disease, though there was no appearance of this last in the lungs, nor in the bronchial glands; neither was there in another case in which an extensive cretaceous deposit was found in the renal capsules, and which was similarly explained.

Several years since my attention was directed to the subject of the infrequency of the occurrence of the tubercular deposit in patients dying from malignant disease, and I was not aware for some time that the same remark had been made by others. Of 33 cases of malignant disease, in nearly all of which a careful examination was made for tubercles, it is expressly stated that in 24 none were found; six times they were found in the lungs alone, and in the bronchial glands alone three times.

Of 35 patients dying of various diseases, all of whom were decidedly intemperate, and most of them grossly so, in 26 no

tubercles were found; in five there were tubercles in the lungs; in one in the bronchial glands; in one in the lungs and bronchial glands; and only two died of phthisis. In several of the most striking, the organs were as free from tuberculous disease as those of a new-born infant. All of these 35 are stated, in the recorded history of the case, to have been intemperate; probably others are so recorded, and the list might have been considerably increased; but no case has been referred to under this head, except where I happened to remember that the patient had been intemperate.

Intemperance certainly does not seem to develope tubercles, even if it has not some effect as a prophylactic; the remedy, to be sure, would be worse than the disease; but has it any such effect?—*New England Quarterly Journal.*

Diabetes Mellitus.—M. Bouchardat read a paper on the treatment of this disease before the Academy of Sciences of Paris, Nov. 15, 1841. The most difficult point in the treatment of this disease is, Mr. B. states, to induce patients to abstain for a length of time, and completely, from all aliments containing fæcula. Although the patient may be firmly convinced that the use of bread is fatal to him, yet, in spite of remonstrance and the utmost vigilance of his attendants, he resumes the nutriment; the diabetic symptoms, which had been moderated during the abstinence from bread, now appear; tubercles are deposited in the lungs, and the patient dies.

In the treatment of diabetes mellitus two important points present themselves for the consideration of the physician; 1. To replace bread by a nutriment containing less fæcula; and 2. To restore the economy to its normal state.

The first of these indications M. Bouchardt has endeavored to fulfil by preparing bread with gluten.. M. E. Martin had succeeded in separating the gluten during the manufacture of starch, but it was necessary to add one-fifth of flour to the gluten in order to make eatable bread. The bread thus prepared is light, and of an agreeable taste, and by using it with animal food the patient will take in little more than a scruple and a half of fæcula during the day.

With regard to the second indication the following considerations guided the author. In diabetes the acid secretion of the skin is suddenly and completely suspended; the mucous follicles and glands of the alimentary canal secrete fluids

which are considerably altered in consequence of the change in the cutaneous transpiration, instead of being alkaline they are acid. But are we to conclude that this acid secretion has any influence on the conversion of fæcula into sugar? Certainly not; for the author had long ago ascertained that acids are incapable of producing any such effect at the temperature at which digestion takes place. Connected with this point, however, there is a fact of much importance. Whenever the organic acids, just alluded to, exist in any great quantity, we have joined to them that modification of albumen which assists in the transformation of fæcula into sugar. The same coincidence probably exists in the system of diabetic patients, and the chief circumstances to which we have to attend are the suppressed perspiration, and the perverted secretion of the digestive canal.

M. Bouchardat next alludes to the various attempts which have been made for the purpose of restoring the cutaneous perspiration; he has found no benefit from the vapor-baths, so highly praised by Dr. Bardsley and others, nor from the sulphureous baths, or the hydrosulphate of ammonia, recommended by Rollo. The best means of restoring the functions of the skin, according to the author, are—1. The use of flannel clothing, in sufficient quantity to keep up constant diaphoresis. 2. The internal use of sudorifics.—*Am. Jour. Med. Sci.*, from *Provin. Med. and Surg. Journ.*, Dec. 4, 1841.

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TWO EVENINGS' EXPERIMENTS ON MESMERISM.

By the Senior Editor.

Having been informed that several gentleman in Cincinnati were practising Mesmerism, and had discovered a number of individuals who were susceptible to its influence, I determined to witness, for the first time, some of its alleged phenomena. Of these gentlemen, Mr. Elijah Burdsal, druggist, was said to be the most successful mesmeriser, and Mr. George Selves, innkeeper, one of those most easily thrown into a state of somnambulism, or what might more appropriately be called somniloquism. On applying to these gentlemen, they obligingly complied with my request, and the time appointed was Saturday evening, May 7th, 1842. Desirous of having the experiments conducted in a manner best calculated to bring out decisive results, I invited William Greene, Esq., and Mr. William R. Foster, who had made themselves familiar with mesmeric processes, and were firm believers in the reality of the effects said to be produced, and that nothing should be left entirely to my own observation and decision, or reported on my own veracity, I requested Messrs. James Hall, Samuel E. Foote and Robert Buchanan, to attend as observers and recorders of the phenomena—these gentlemen having assured me that they had made

up no opinion on the subject of mesmerism. Messrs. E. D. Mansfield, Peyton S. Symmes, Alexander H. McGuffey and Dr. John A. Warder, and several ladies, were present as spectators.

On the arrival of the different gentlemen, at the house of Mr. McGuffey, I stated that my object would not be to ascertain the reality of the mesmeric sleep, *which I should grant*, but, whether Mr. Selves, while in that condition, could feel impressions made on my organs of sense and general feeling, when I should be placed in connection with him.

On examining the pulse of Mr. S., immediately after walking, it beat 96 in a minute; his health was good; his age between 30 and 40; his temperament lymphatic.

Mr. Burdsal commenced his manipulations 10 minutes after 8 o'clock; and in five minutes pronounced Mr. S. asleep. My left hand was then placed in his, and after a few manipulations I was directed to put a question to Mr. S., which he answered, when Mr. B. pronounced the connection perfect. For the next hour and a half our hands remained in unbroken contact, and I stood by his side. Before any experiments were commenced, and when Mr. S. was not touched, it was observable, that his muscular system was unquiet, and that he moved his limbs, more or less, and also the muscles of his face, giving to his features more or less of distortion.

Experiments on Muscular Motion and Muscular Sensation.

1. Standing so far behind the subject, that if his eyes had not been closed, I could not have been distinctly seen, I took up one of the gentlemen with my right arm, and held him as long as it was possible; but Mr. S. gave no indication of sympathy with me in the effort. He did not exhibit more muscular uneasiness than before the experiments, and when interrogated did not complain.

2. One of the gentlemen then placed himself on my shoulder, and at length Mr. S. seemed to shrink away, and raising his left hand, appeared to point towards his shoulder. On being questioned he answered, inarticulately, but in tones which indicated oppression or uneasiness.

Experiments on Respiration.

1. Standing by his side, I breathed with great rapidity for about a minute, during which he raised his left hand toward his head, and also extended his arm; but his breathing remained tranquil.

2. In the same position, I suspended my breath for a minute, at the end of which time I felt deep anxiety in the chest, but he remained quite tranquil.

Experiments on the sense of feeling in reference to pain.

1. My right arm was rubbed with sand paper, when he gave evidence, by his restlessness and murmurs, of feeling pain; the sand paper, which made a noise, was then rubbed on the coat sleeve of a gentleman by my side, and my own arm was rubbed with a piece of fur, which appeared to affect him in the same manner as the sand paper; indeed his contortions and writhings at length became very great, and continued as long as the friction on the coat was kept up.

2. Dr. Warder then made a great number of punctures with a lancet on my arm, so deep as to bleed, during which the subject displayed some contortions of the mouth, and on being questioned, said it tasted like tobacco.

3. One of the gentlemen then bore his weight on the great toe of my right foot—protected only by a sock and woollen slipper—until I could no longer endure the pain. The countenance of Mr. S. remained quiet, and he did not complain on being questioned, but was observed to raise his right foot and turn his toes inward as he had done before.

Experiments on the sensibility to caloric.

I held my hand and arm for some time in contact with a mass of ice. He manifested, when questioned, no feeling of coldness, but showed slight muscular uneasiness and contortions of the mouth. At length when the pain of coldness had become very severe, he became quite composed, and said, on being questioned, that he felt well.

2. I held in my hand a smoothing iron, so hot that it raised a blister, and could not be borne by Mr. Greene, on whom it also rested. It produced on Mr. S. no particular effect; some contortions of the mouth only occurred.

Experiments on colors.

1. He being closely blindfolded by Mr. Foote, a broad lead colored surface, strongly illuminated, was placed before me, and I gazed intently upon it, when he, on being asked its color, pronounced it "green." I then looked at a bright red, and he declared it, first, a "light color," then "white." To this succeeded a white surface, which he pronounced "yellow." Then a black, on which he said, "don't know"—"dark"—"something on it"—"dark."

I now nearly closed my eyes and turning them towards the ceiling of the room, endeavored, without involving myself in darkness, to exclude every vivid color, and also sought to have my mind occupied on the idea of color in the abstract, and not on any particular tint: his first answer was, "light"—then "sun"—then "pink." This experiment could be verified by the gentleman present, in this, that he answered "pink" when nothing of that color was in sight, but *they* could not know but what the idea of pink was in my mind; and therefore, although I have incorporated it into the report, I do not ask it to be put on a level with the other facts. In such an investigation as this, the declaration of no individual as to what was or was not passing in his mind, should be received as incontrovertible.

Experiments on the sense of taste.

These were sixteen in number, and so conducted, that no one present but myself knew, except in two or three instances, what I tasted, until the whole were completed. The different glasses used in the experiments were numbered, and the contents of each were registered in a sealed paper, which, after the tasting was over, was opened and found by the examination of Mr. Foster and Mr. Buchanan to be correct. I rinsed my mouth after each experiment, and sometimes used a brush—in short, did not proceed to a subsequent glass, till the taste produced by a preceding, when, indeed, there was any, had died away. Some of the glasses were empty.

1. Milk—"*Good*"—"*don't know.*"
2. Water—"*Don't like it*"—"*sour.*"
3. Empty—"*Well*"—"*doesn't taste like any thing*"—"*don't like it.*"
4. Wine—"*Don't taste*"—"*don't know.*"
5. Peppermint water—Muscular action of the face—"*Don't know.*"
6. Syrup of loaf sugar—"*Very good.*"
7. Water—"*Wine.*"
8. Vinegar—On being asked by Mr. B., "Is it sour?" answered, "*Yes.*"
9. Brandy—"*Don't like it*"—"*don't know*"—"*makes me sick.*"
10. Sweet cake without fruit—"*Likes it.*" On being asked what kind of fruit it was, answered—"*Resembles raisins.*"
11. Infusion of nut-gall—"*Good*"—"*don't like it.*"
12. Strong salt and water—"*Tastes nothing*"—"*water.*"

13. Piece of cold chicken—"*Don't like it.*"

14. Radish—"*Nothing.*"

15. Empty glass—Contortions of the mouth—"*Don't like it.*"

16. Strong, bitter infusion of Colomba root—"*Good*"—"*tastes sweet.*"

Second Series of Experiments, Saturday night, May 14.—Present Mr. Selves, to be mesmerised, and Mr. Burdsal, Mr. Foster, and Mr. Thos. J. Matthews, professed believers in the extraordinary powers and susceptibilities of one in a state of mesmeric sleep; also Messrs. Hall and Buchanan as reporters of the phenomena, and Dr. Warder, Mr. Symmes, and Mr. McGuffey, as spectators, with a few ladies.

The Mesmerising.

At 31 minutes past 8 o'clock, Mr. S. took his seat, with a pulse beating 80 in a minute. Under the direction of Mr. Burdsal, I undertook to throw Mr. S. into somnambulism, by pressing my thumbs upon his, and looking him steadily in the eyes. In 4 minutes he was asleep, with considerable rigidity of his limbs; his breathing natural. He was then blindfolded.

First Experiment.

My design was to observe his movements when there was perfect silence in the room, and he was left to himself untouched. I did not make any effort to act on him, but took a chair a few feet behind him, to watch. I then went into the adjoining hall, and afterwards out of the house. He slowly raised, first one hand, and then both; then made an effort to rise from his chair; spread out his arms; leaned forward, and had contortions of the face. This experiment lasted ten minutes.

Second Experiment.

I now caused Mr. Mansfield to take hold of his hand and press it in a variable manner; he also rubbed the hand, wrist and head. I remained in the room to witness the effect. He raised the left foot, and then the right, and kept both about ten inches from the floor. This experiment lasted 5 minutes.

Third Experiment.

I called audibly to Dr. Warder to be put in connexion with Mr. S.; he came forward, but I placed Judge Hall's hand, instead of the Doctor's, in Mr. S.'s, and made the prescribed passes, to connect them

together. Dr. Warder then put *several* questions to him, which he answered. The Judge did not speak, and the Dr. did not touch him; at length Mr. Burdsal said, he is not answering Dr. Warder, after which we could get no further replies.

Fourth Experiment.

This was on taste. I was myself the taster, no person save Mr. McGuffey and Mr. Foster knowing what I took. Three successive trials were made with each of the following articles, it being tasted anew twice. In the first trial, I put no leading question—in the second the questions led off towards some other article—in the third, they led towards the article, or in a different way. What seemed to be ample time for distinctness was allowed. The following were the answers, as recorded by Mr. Buchanan, and distinctly heard by all who were near him. The articles were numbered. The answers in the first column were obtained without leading questions; those of the second and third columns by leading questions—the former directed from, the latter to the article tasted.

1. Bread and butter.	Screws up his mouth and smacks his lips—'*Sweet.*'	'*Don't like that.*'—Same movements of the face.	'*Tastes like bread.*' Less movement of the face.
2. Hydrant water.	Screws up his mouth —'*Water.*'	Twists his mouth—'*Weak.*'	But little movement of the face—'*Strong.*'
3. Boiled ham, cold.	Face placid, smacks his lips—'*Good;*' '*don't know.*'	Twists his mouth—'*Don't know;*' '*don't like that.*'	Smacks his lips—'*Can't taste that.*'
4. Hydrant water.	Contortions of the face & mouth—'*Don't like it.*'	Contortions of the mouth—'*Don't like it;*' '*tastes salt.*'	Smacks his lips—'*Don't know.*'
5. Boiled peas, warm.	Face placid—'*Yes;*' '*good.*'	Smiles, then screws his mouth—'*Don't like that.*'	Contortions of the face—'*Don't like that.*'
6. Hydrant water.	Face placid—'*Yes;*' '*very well.*'	No change of expression—'*Can't taste that.*'	Contortions of the face—'*Nothing.*'
7. Boiled egg.	Hard compression of the lips—'*Don't like it;*' '*don't know.*'	Lips compressed—'*Don't like it;*' '*tastes bitter.*'	No expression of face—'*Can't taste that.*'
8. Hydrant water.	Looks pleased—'*Good;*' '*don't know.*'	Face placid—'*Water.*'	Same expression—'*Can't taste that;*' '*can't tell.*'

Fifth Experiment.

This was on sight. Placing my back towards him, I looked intently on four successive articles, in a strong light.

The first was a common counter dust brush. Quest. Do you see what I am looking at? Ans. "*Something there.*" To other questions concerning it, not of a leading kind, his replies were—"*It opens*"—"*it pulls out there*"—"*something on there,*" and "*I don't know.*"

The second article was a red morocco razor case, with the cap off, and one of the razors partly drawn out. His replies to various questions were—"*It looks bright*"—"*on there*"—"*on that*"—"*on the table*"—"*I don't know.*"

The third article was a small plaster cast of a head, designed for phrenological purposes:—His answers to different questions were—"*Down there*"—"*round*"—"*don't know*"—"*hard*"—"*don't know.*"

The fourth article was a small, red carpet bag. Answers to various questions—"*Can't see that*"—"*don't know that*"—"*something on there.*" To the question, do you see me? ans. "*Yes.*"

Sixth and last Experiment.

Standing behind Mr. S., I took Mr. McGuffey on my shoulders. Quest. What do you see? "*Don't like that*"—"*it hurts.*" Where? "*Down there,*" and appears disturbed. Does it hurt your foot? "*Yes.*" The left foot? No answer. Becomes more quiet. Would you like to go home? "*Yes.*" Have you eat or drunk more than you wanted? "*No.*"

His pulse at this time was 80 in a minute and regular. On raising his eyelid, the pupil was dilated, but contracted under a strong light. Awakened after an hour and forty minutes.

It will, I think, be granted, that in these two series of experiments Mr. S. did not give evidence of sympathising with me in my bodily sensations. The few answers which seem to favor the conclusion that he did, may, I think, be otherwise explained. 1. If a man blindfolded, should have been called on to answer similar questions, according to the doctrine of chances *some* of them would be correct. Thus, when four colors were successively observed, and there were scarcely more than twice that number to draw answers from, it would have been very remarkable if all should have been erroneous, yet he named but one of the four, and did not decline speaking of the others, but declared them different from what they were; and when on the

first night three kinds of food were tasted each once, and on the second, four kinds were each tasted three times, making fifteen in all, it might have been expected that much more than one would have been correct; but such was not the fact, and even that one was imperfect, for when I tasted bread and butter, he only said, "*tastes like bread,*" but immediately before had said of the same article, "*don't like it.*" Again, water was tasted, in the experiments of the two nights, fourteen times, and as it was announced to be a drink, we might expect that the answer would be correct once or twice. It was, indeed, correct twice; but immediately after one of these answers, he said, on another tasting, "*strong;*" and in other experiments with water from the same hydrant, he said, "*don't like it,*" "*tastes salt.*" In another experiment he said of it, "*don't like it*"—"*sour*"; in another, "*wine.*" Of milk and a syrup of loaf sugar he said "*good,*" "*very good,*" which would seem to indicate, that he was impressed by what acted on my organs of taste, but I am inclined to put these answers down to chance, when I recollect that he said of a very strong solution of common salt, "*taste nothing*"—"*water*"; of a boiled egg that it was "*bitter,*" and of an intensely bitter infusion of Colomba root, "*good*"—"*tastes sweet.*"

2. Some of his answers, as would be those of a person blindfolded, appeared to be brought out by the mode of interrogation. Thus when I had vinegar in my mouth, Mr. Burdsal, *by accident* (for he did not know), asked him if it was sour? and he answered "*yes,*" but still did not give the name. Again, when I was eating cake, and asked him what *fruit* it was like, he answered "*raisins.*" Again, when my eyes were partially closed, and nothing of a definite color acted on them, but I kept on asking what color I saw, he answered "*pink.*" Again, in the experiments of the second night, when I tasted the same articles three successive times, changing my *mode* of interrogation each time, his answers changed likewise, so that in no case were they uniform. Now I think these variations in his replies can only be ascribed to variations in the mode of questioning.

3. His apparent sufferings under some of the inflictions on me, may be explained on the principle of coincidence. As soon as he was put to sleep, the second night, and left to himself, no one touching him, and the room in perfect silence, he commenced and maintained a series of muscular movements of his face, arms, legs, and body. These then may be regarded as phenomena running through all the experi-

ments, and sometimes coinciding with them in time, so as to seem a part of them. Thus when I had a heavy weight on my shoulders, and nothing was said to him about taste, the muscular movements which he exhibited might have occurred, if no such weight had been borne by me; and when a gentleman rested his weight on my great toe and stood silent, and he moved his foot, he might have done it, if I had not been acted on, seeing that such motions occurred spontaneously several times. These automatic muscular movements were, in fact, a kind of constant quantity, running through all the experiments, and by coincidence and contrariety, harmonising with some and proving discordant to others, and to be rejected from the results of the whole.

4. It is probable that some of his answers were modified by what he heard around him. That he did hear those who were not in connection with him the first night, seemed almost certain, from the fact that his contortions, writhings, and signs of pain, became more and more violent, the harder a gentleman rubbed his coat sleeve with sand paper, while my arm was rubbed with a piece of fur. But the second night it was *demonstrated* that he heard, for he answered several questions put by Dr. Warder, whom he believed to be in communication with him, when he was not.

Thus I have pointed out four circumstances, by which we may explain the few correct replies obtained from him; and, I repeat the conclusion already stated, that he did not feel the impressions made on my system. But I go *no* further; I do not say that he has not felt those made on the systems of other persons; I do not say, that other mesmerised persons do not feel all that is felt by those placed in communication with them. I do not say, because Mr. S., under my experiments, could hear those *not* in connection with him, that he can do it at other times, or that other persons in mesmeric somnolency can hear what is spoken in their presence. I disclaim all intention of drawing conclusions relative to him at other times, or others at any time.

D.

THE COLLEGE OF PROFESSIONAL TEACHERS.

We have just received the Minutes of the eleventh session of the *Western Literary Institute and College of Professional Teachers*, held in Cincinnati, the place of all the other meetings, in the month of October last. By these proceedings we learn, that the next meeting will

be held in the city of Louisville, on Monday, the 15th day of August. As this will be the first meeting in our city, we hope to see it well attended by gentlemen from a distance, not less than our own citizens. Medical gentlemen, who do not reside very distant from us, and can leave their patients, will be well rewarded for an attendance on the meeting. They are deeply interested in the promotion of sound elementary learning, without which we shall not have students qualified for the study of medicine, nor graduates fitted to do honor to the profession. D.

INFLUENCE OF AUSCULTATION ON PRACTICAL MEDICINE.

We hope our readers will not suffer the length of Dr. Pope's translation to deter them from reading it; it will richly repay them for an attentive perusal. At all events it will enable them to answer the question so frequently asked: what good has auscultation done? The whole essay, of which the present number contains about one-half, strikes us as being a learned and ingenious one. As an evidence of its merit, we may mention that the Bordeaux Medical Society not only awarded to the author, Dr. Peyraud, its prize, but, as a further mark of distinction, conferred upon him the title of Corresponding Member. We shall endeavor to publish the remainder in our next.

Dr. Pope has recently returned from Paris, having spent two or three years in the assiduous observation of disease in the hospitals, and in attendance on the lectures of the distinguished men with whom that capital abounds. He has taken up his abode at St. Louis, where we learn he is rapidly advancing in public confidence. We spent part of our novitiate with Dr. P. and know him intimately. If a high and honorable ambition, joined to fine talents and an industry that knows no obstacles, can insure success, no man will meet with a greater measure than he. C.

CORRECTION.—Dr. Monette has requested us to make correction of an error that crept into the MSS. of his last article on yellow fever, published in our preceding number, the entire edition of which had passed through the press before his letter was received. On page 407 (vol. 5, no. 6), instead of the first sentence of the third paragraph, substitute the two following:

"5 Opelousas is a large town or city, beautifully spread over an extensive area, upon a dry, rolling prairie, and proverbial for health. The houses are not crowded, and the streets are spacious and cleanly, and destitute of all the causes which are said to generate yellow-fever miasm."

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ART. I.—*A History of the Improvements which Practical Medicine has derived from Auscultation; being the essay to which the Medical Society of Bordeaux awarded its prize in November*, 1839. By G. PEYRAUD, Doctor of Medicine of the Faculty of Paris, Corresponding Member of the Bordeaux Medical Society. *Quæ fundata sunt in natura, crescunt et perficiuntur; quæ vero in opinione, variantur, non augentur.*—Baglivi. Translated from the French for the Western Journal of Medicine and Surgery, by CHARLES A. POPE, M. D., of St. Louis, Lecturer on Anatomy and Surgery, Physician to the St. Louis Dispensary, &c., &c.

(*Concluded.*)

CHAPTER IV.—OF DISEASES OF THE HEART.

Thus far my province has been to state the real progress which the science of diagnosis owes exclusively to auscultation; I have shown that this admirable discovery has rendered it as easy to acquire a knowledge of the diseases of the lungs and pleura, as it was formerly difficult. Here my office chan-

ges, and instead of an exclusive admiration of the results obtained, I shall be led to inquire whether positive results have been really established, and whether the pathology of the heart has gained any thing by auscultation. But before entering on the discussion of this question, which, I confess, may appear to many persons a true paradox, let us see where the science was, relative to diseases of the chest, when Laennec began to apply his new method of exploration to their study.

Of all the discoveries which physiology has ever made, the most brilliant, beyond contradiction, and that which has exerted the most influence on pathology, is the discovery of the circulation of the blood. Besides the new light which it shed on one of the most important functions of the economy, it also proved that the ancients did not know and say every thing. The magic which surrounded the great name of Galen once destroyed, physicians became more bold, and no longer restricting themselves to the sterile and ungrateful task of commentators, entered the vast field of observation, where such rich harvests awaited them. Of all parts of pathology, the one which was necessarily to gain most by this scientific revolution, was that relating to diseases of the heart, which organ Harvey had just shown to be clothed with functions so important, and up to his time unknown. Before this ever celebrated epoch in the fasti of the art, the study of these diseases had been completely neglected, and if some scattered observations are met with here and there in the authors anterior to the seventeenth century, it is only as rare, curious, and extraordinary facts that they are reported, and not at all with the view of their co-ordination to refer them to a theory. But when once Harvey had made known his beautiful discovery, in 1619, the animated discussion which arose on this question, led to important researches, all of which profited the pathology of the heart, by throwing more and more light upon its structure, and the mechanism of its functions. From that time Lower, Ruysch, Vieussens, Leuwenhoeck, and Haller, multiplied these researches and experiments so as to leave scarcely any thing more to be discovered in the anatomy and physi-

ology of the heart; whilst Lancisi, Albertini, Bonnet and Valsalva successively enriched its pathological history with a multitude of curious facts, which it only remained for Morgagni to complete, reunite and compare, with his remarkable sagacity, to make of his seventeenth and eighteenth letters, which he has devoted to aneurisms of the heart and of the aorta, one of the most remarkable parts of his great work.

It does not enter into my plan to analyse the writings of these different authors, in which, not even excepting Morgagni, I would find many anatomical details, but very few data calculated to make the diagnosis plainer. I shall restrict myself to two writers whose works seem to me to sum up pretty well what has been best said on diseases of the heart, before Laennec: I mean Sénac and Corvisart.

The *Traité de la structure du cœur, de son action et de ses maladies*, by Sénac, published in 1749, is certainly the most remarkable which had appeared at that time on this important subject. This immense work is divided into six books, the last of which only is devoted to diseases of the heart. It is therefore the only one that I shall notice, whatever be the merit of the first five, in which the author treats at length of the anatomy and physiology of the heart, and their merit is so great that it required nothing less than the incomparable éclat of the works of Haller to eclipse them a little at the epoch when they appeared.

The idea that predominates in the first chapters which Sénac devotes to diseases of the heart, is the extreme difficulty of their diagnosis. After having shown that diseases foreign to the heart simulate the latter so closely as almost infallibly to lead into error, he adds: "But when by distinguishing between the symptoms of other parts and those which arise from the heart, we ascertain the latter to be diseased, new difficulties occur: where is the cause of the disorder? It may be in the ventricles, in the auricles, or in the various parts enclosed by them; this is all we know about it: we are ignorant whether it is in the right cavities or in the left; the symptoms, which ought to vary according to the parts which produce them, are

not different; they reduce themselves to palpitations, trembling, inequalities, in a word to a deranged action." (Senac, op. cit., vol. ii, p. 315).

We see whether Sénac dissembled the difficulties of the question. Perhaps, indeed, this emphatic declaration was a condition of success, for the questions which are most deeply penetrated, are those which have been thus examined in all their phases. Our author therefore courageously undertakes the solution of the problem which he proposes, and we cannot but admire the sagacity he evinces in separating the uncertainties from it. He considers, in the first place, the type of the disease; and from its intermittent or continued character, from the permanency of the symptoms or their momentary cessation, he infers the existence of organic lesions or the healthy state of the heart. The pulse also furnishes him important signs: its constant smallness and irregularity denote an alteration of the auricles or ventricles; the contrary state proves that the heart is not altered in its organization. If the symptoms are developed after a violent passion, as anger, or after a strain or blows on the sides, some disease of the heart is to be feared; whilst, if this organ has been agitated from a slight cause only, for example, the suppression of the catamenia, it is probable that the nerves create the disorder in its movements. The duration of the disease is likewise an important circumstance: thus the reappearance of dropsies is a strong reason for suspecting a disease of the heart. Lastly, if it be impossible to recognise certain diseases in particular, they may be known in general, and, although it cannot be determined whether the valves be ossified or whether there be a tumor in the ventricles, still it is not a matter of indifference to know that the heart is diseased.

I cannot but confess that these elements alone do not suffice to enable us to arrive at a certain or even probable diagnosis of a lesion of the heart; but it is not Sénac's fault, and he deserves praise for having learned how to profit by them as much as he has: there was but one practitioner so enlightened who could thus artificially create a diagnosis from inductions

whose basis is apparently so slight. Besides, when the occasion presents itself, he does not neglect to draw signs elsewhere than from these data. Thus, it was he who pointed out the fluctuation of the liquid between the fifth and sixth ribs, in certain cases of hydro-pericardium; a sign which does not occur so frequently as he supposed, but which Corvisart and M. Bouilland have since met with.

I pass over the chapters which he devotes to inflammations of the heart, and to the study of various other lesions, such as vegetations, osseous or calcareous concretions of the valves, displacements of the heart, etc., chapters remarkable for the great number of facts which are found therein, and for the judicious criticism to which these facts are submitted, but which present nothing of interest as it respects diagnosis, which was as difficult, or rather, as impossible for Sénac, as it yet is for us; and I come to that in which he treats of aneurisms of the heart.

The signs on which he seeks to establish the diagnosis are the following: dull and heavy pains in the region of the heart; difficulty of respiration, which he attributes to the depression of the diaphragm by the dilated heart (the constraint of the lungs would have been a much more natural explanation); small, quick, and irregular pulse; universal palpitation of the whole arterial system, which is felt at the same time in the head, arms and thighs (Sénac grants indeed that this sign does not belong exclusively to aneurisms of the heart, and that he has observed it in other diseases of this organ); beating of the jugulars, swelling of the extremities, and sometimes effusion in the cavities. (Op. cit., vol. ii. p. 473, et seq.) Sénac had too much judgment not to perceive, that there was not one of these signs which did not equally belong to all the diseases of the heart; therefore he soon adds another which, he says, *is more than conjecture and probability:* "If the hand applied to the region of the heart, feels a mass which strikes the ribs in a great extent, it is a decisive proof of dilatation." (Op. cit., vol. ii, p. 485). But in cases of simple hypertrophy of the heart without dilatation of its cavities, the heart strikes the ster-

num with a strong and quick stroke, which may be taken for that of a mass, on account of the vibration which it communicates to the whole sternum. It is true, that Sénac did not yet distinguish hypertrophy from simple dilatation, a distinction which has not been well established until since Corvisart's time.

Farther on, endeavoring to establish a differential diagnosis between aneurisms of the heart and those of the aorta, he reduces the characteristic signs of the latter to three principal ones: 1. seat of the pulsations in some other point of the thorax than the præcordial region, and which varies according to the part of the aorta which is diseased; 2. pains much greater in aneurisms of the aorta, than in those of the heart; 3. production of exterior tumors by the first, which, when they are voluminous, cause absorption of the ribs and project without. (Op. cit., vol. ii, p. 489) Since Sénac, no new differential sign has been indicated, and it must be avowed that when they present themselves united, which unfortunately does not in all cases happen, they must certainly throw great light upon the diagnosis.

Here I terminate the analysis of the work of Sénac, and I think I may say, in conclusion, that it is the most complete one which had yet appeared on this subject, and that it would be still almost on a level with the science, but for the labors of Corvisart and of the modern anatomical school.

The work of Corvisart (*Essai sur les maladies et les lésions organiques du cœur et des gros vaisseaux*), is in the first place distinguished by the application of the method of Avenbrugger to the diagnosis of those diseases, which at once gives it a marked preeminence over every thing which had been published up to that time. This mode of exploration is so natural for affections of the heart, that I should not be loth to believe it was in devoting himself to their study that Avenbrugger discovered it. Where indeed shall we find a more striking contrast than that which exists between the dulness of the precordial region, and the sonorousness of the neighboring part of the chest? The limits within which these various phe-

nomena take place, is always precise, and where the flatness ceases, the sonorousness commences, without there being an intermediate space where they are confounded. The reason is simple: the heart, a thick, massive organ, is immediately enveloped on all sides, except in front, by the lungs, which always contain air (in the healthy state, I mean), and consequently are always light and crepitant. The enlargement of the space where the precordial flatness is heard, becomes therefore an important sign, especially when combined with the other rational signs, of which Corvisart has given a more correct and profound explanation than had been previously offered.

I know, forsooth, that some objections may be made to percussion applied to the diseases of the heart. The volume of this organ varying with individuals, there must be an uncertainty as to whether the extent of the space in which the flatness is found, be normal or otherwise. But what physician will not be able to distinguish whether this extent surpasses the reasonable and probable volume of the heart, in the patient whom he examines? And besides, the variations in the volume of the heart never reach the limits which would be considered pathological by the practitioner, who does not think of percussing the chest until the general symptoms have already fixed his attention. In the second place, will percussion permit us to distinguish an enlargement of the heart from an accumulation of serum in the pericardium? Certainly not, if we have reference to its results alone. But explore with the hand the pulsations of the heart, study its rhythm and compare it with that of the pulse; consult the general and sympathetic symptoms which the patient presents, and you will approach some degrees nearer the truth. What then is the method of exploration which alone can replace all others, and which does not require to be combined with them, to afford more certain results? Auscultation itself, the one whose results have been the most brilliant, cannot do without this union with the others; how much less, then, should percussion!

Again, Corvisart is far from confining himself exclusively to

percussion, and he studies the other symptoms with a care unknown before his time. Read what he says of the *facies propria* of patients affected with a lesion of the heart; of the engorgement of the venous system throughout the whole body; of the jugular pulse, which he wishes to be well distinguished from that of the subjacent carotids; of the pulsations of the heart, often perceptible to the sight, and accompanied by a *sort of roaring* (*bruissement*) *or peculiar disorder of the circulation when there is contraction of the orifices* (does this not anticipate the bellows sound (*bruit de soufflet*) of Laennec?); of the prominence of the walls of the chest, which he first pointed out under the name of *arching* (*voussure*) of the thorax; of the engorgement of the liver, which so often accompanies these lesions; of the swelling of these limbs, caused by the blood not being able to return to the abdomen (Corvisart, op. cit., p. 373 et seq.); and say whether he has neglected any of the elements suitable for conducting him to the object of his researches—to recognise during life the diseases of the heart. He has not indeed removed all difficulties, he has not made all uncertainties disappear; these difficulties and uncertainties still exist, and we shall shortly inquire whether auscultation has produced any very satisfactory results, for the solution of all these questions.

From all that I have just said, I believe myself right in concluding, that before the time of Laennec the theory of the diseases of the heart had been pushed farther, and was more advanced than that of certain diseases of the lungs and pleura, and that it was also easier to recognise an aneurism of the heart, than certain chronic pleurisies and consumption. What was not to be expected, then, as the complement to the art of diagnosis applied to those diseases, from the brilliant discovery of Laennec! Alas! these flattering hopes have been disappointed; and if the pathological anatomy of the heart has made recent improvement, the diagnosis of those diseases has remained at or near the point where Corvisart left it.

Whence this immense difference between the results of auscultation applied to diseases of the heart on the one hand,

and to those of the lungs on the other? Why so much uncertainty in the former, and so much certainty in the latter? All arises from the difference in the starting point (*point de départ*). Auscultation of the lungs, indeed, was founded on this simple and exclusive basis, that every modification of the normal sound of respiration, denoted some obstacle to the respiratory act being executed as in the healthy state. This admitted, it was only necessary to find anatomically to what lesion such a sound or râle, brought to the ear of the observer by the stethoscope, corresponded. This has been effected: and if the new symptomatology which has resulted from it, has been found to be immovably certain, it is because the principle on which it was founded is itself beyond controversy. The respiratory murmur could only be attributed to the dilatation of the pulmonary vesicles by atmospheric air: so evident is this, since it results from the very functions of the lungs, that no other explanation could be given. This respiratory murmur, always the same in the normal state, could only be modified in an uniform manner by the same lesion in whatever individual it should be observed, and this modification of the sound could only be in constant relation with the nature of the obstacle which this lesion induced in the respiratory function. Hence, as I have said, the certainty of the new signs which Laennec has appointed to diseases of the chest.

But for the diseases of the heart, there is unfortunately nothing similar; and, in the first place, there is no fixed starting point to enable us to interpret in a certain manner the modifications which the sounds of the heart may present. In short, to assign a certain and real value to these modifications, it would be necessary to agree on the cause of the sounds, and on this point there are almost as many different opinions as there are authors.

Laennec has no where given his views respecting the cause to which he attributed these sounds; but it is easy to see, from the complacency with which he analyses the experiments of Erman and Wollaston on the sounds produced by the contraction of certain muscles, that it is to the contraction itself

of the ventricles and auricles, that he attributes the double sound which accompanies the beatings of the heart. According to him, the first and duller sound, synchronous with the pulse, is produced by the contraction of the ventricles; the second, sharper (*éclatant*), and immediately following the first, results from the contraction of the auricles; and the short, very well marked interval of time, which separates this double sound from that which follows, corresponds with the repose of the heart. (Laennec, op. cit., vol. ii, p. 404, et seq.)

This theory, which for a long time met with no opposition, as if the great name of its author had served as a bulwark against all criticism, was slightly modified by M. Marc Despine who, in a memoir read before the Academy of Medicine, the 29th July, 1831, arrived by exclusion at this result: that the first sound was owing to the contraction of the ventricles, and the second to their dilatation. But after all, the original idea of Laennec's theory still existed, and the contraction of the fleshy fibres of the heart was recognised as the cause of the sounds of this organ. All at once this theory found opponents; and so soon as the signal was given, new theories were started in rivalry, agreeing among themselves only in the complete subversion of the ideas of Laennec.

M. Pigeaux was the first who commenced this kind of scientific insurrection against the illustrious author of auscultation, in a thesis defended at Paris in 1832, entitled: *Diverses propositions relatives à la physiologie et à la pathologie du système circulatoire;* and in a memoir entitled: *Examen critique et comparatif des divers systèmes qui ont été émis sur la cause des bruits du cœur.* (*Archives générales de Médicine*, vol. xxx, p. 356, November, 1832). The object of M. Pigeaux in these different works was to show that the first sound, which he calls the *inferior*, was produced by the shock of the blood thrust by the auricle against the walls of the ventricles; and that the second or *superior sound*, was produced by the shock of the same liquid, against the walls of the aorta and of the pulmonary artery. But in announcing this opinion, M. Pigeaux did not reflect on the flagrant contradiction which

it gave to facts. For, if the first sound be due to the shock of the blood against the walls of the ventricles, and correspond consequently with the contraction of the auricles, how happens it that it is synchronous with the pulse? As the arterial pulsation must evidently coincide with the sound resulting from the contraction of the ventricle, it should be synchronous with the second or *superior sound*. Now, that the contrary takes place cannot be denied, and this single fact annihilates the theory of M. Pigeaux. Hence, in p. 49 of the work recently published by him under the title of *Traité pratique des maladies du cœur* (Paris, 1839), this author has modified his theory so as to make the first or *inferior* sound arise from the friction (*frottement*) of the blood against the parietes of the ventricles, the orifices and walls of the great vessels; and the second or *superior* sound, from the friction of the blood against the walls of the auricles, the auriculo-ventricular valves, and the cavity of the ventricles. This new theory, which completely removes the objection that I have just named, has besides the merit of agreeing pretty well with the mechanism of the contractions of the heart, such as it is admitted by all physiologists. But this is not sufficient to convert it from a mere scientific opinion to an incontrovertible truth, and I shall not consider the arguments by which the author pretends to arrive at that result.

Dr. Spittal, whose experiments relative to the crepitant râle of pneumonia I have already mentioned, has put forth an idea nearly similar to that of M. Pigeaux, on the cause of the sounds of the heart, which he likewise explains by the friction of the blood against the cardiac walls. According to this author, the movement of the fluids on the solids determines the production of the sound; and he founds his opinion on an experiment in which, placing the ear on a small leaden tube, which conducted water to a faucet, every time that the latter was opened he heard a very distinct rubbing sound, whose intensity was always in proportion to the degree of the opening of the faucet; and by opening and closing it alternately, he obtained a sound altogether similar to the rasping sound sometimes heard in certain diseases of the heart.

Dr. Hope, author of an excellent treatise on diseases of the heart, published in 1832, has adopted an opinion on the cause of the sounds of the heart, which differs from the preceding, inasmuch as it refers these sounds to the collision of the columns of blood in the interior of this organ. At the moment of its systole, the layer of liquid in contact with the ventricles receives an impulsion which, by propagation from particle to particle, produces the first sound. This is reinforced by the collision of the numerous currents which escape from the interval of the columnæ carneæ of the heart. At the moment of the diastole, the blood coming from the auricles rushes with violence into the ventricles; but the dilatation of the latter being suddenly stopped, this determines a reaction against the liquid admitted into the ventricular cavity: hence the production of the second sound.

About the time of the publication of the writings that I have just named, M. Rouannet maintained, in a thesis entitled: *Analyse des bruits du cœur* (Paris, 1832), that the sounds of the heart are owing to the play of the valves of that organ, the first sound being produced by the auriculo-ventricular valves, during the contraction of the ventricles, and the second taking place at the moment when the elastic reaction of the aorta and pulmonary artery replaces the sigmoid valves of these vessels. This theory, the idea of which M. Rouannet derived from Carswell, and which was adopted by M. Bouillaud in his *Traité clinique des maladies du cœur* (1835), and by M. Ph. Bérard, in his article *Cœur* (*physiologie*) in the 2d edition of the *Dictionnaire de Médecine* or *Répertoire général des Sciences Médicales*, is supported by reasons and experiments which render it infinitely more probable than the others. However, I should remark, that it is open to a serious objection, of which its adversaries have taken great advantage: it is that, though explaining pretty well the normal sounds of the heart, it is insufficient for the pathological sounds, which cannot, in scarcely any case, be referred to the play of the valves. M. Rouannet and his partisans are therefore forced, in order to make these abnormal sounds agree with their theory,

to admit another element in the production of the sounds of the heart, viz. the friction of the blood against the walls of the cavities containing it. This objection is more specious than solid, but it is not my business to refute it; I will only draw from it a new proof that there is no evident and indisputable connection between the sounds of the heart and the cause which produces them, since we have already stated on this point very many opposite opinions, not one of which presents those convincing characters which unite all suffrages.

But we are not at the end of all these theories, and I continue the review. M. Magendie read before the *Académie des Sciences*, at its sitting of 3d February, 1834, a memoir, in which he endeavors to prove that the double sound of the heart is produced by the successive collision of the apex and the base of this organ against the anterior wall of the thorax. The theory of M. Magendie was destroyed as soon as published, by M. Bouillaud, who having laid bare the heart of a cock and that of a rabbit, and having completely separated the organ from the solid parietes of the chest, distinctly heard the double sound. (Bouillaud, op. cit., vol. i, p. 123 et seq.)

Finally, M. Piorry has inserted in the *Archives Générales de Médicine* (June 1834) an article in which, refuting by contrary experiments those on which M. Rouannet has founded his theory, he arrives at this result: 1. that the play of the valves does not produce the sounds of the heart; 2. that the sounds of the right heart are louder than those of the left; 3. that he is tempted to attribute the dull sound to the contractions of the left heart, and the clear sound to those of the right. In writing this, M. Piorry forgot that the contractions of the ventricles are simultaneous, and that the two sounds are not.

My object in thus enumerating at length the different theories which have been promulgated on the cause of the sounds of the heart, has not been to discuss which is the best, but only to show how widely they differ from each other. It was, however, indispensable to agree on the cause of this sound, in order to interpret in an uniform manner the modi-

fications perceptible by the stethoscope. We shall, therefore, find the same discrepancy among authors, on the value of the abnormal sounds of the heart.

All these abnormal sounds consist of the bellows murmur, the rasping or sawing sound, and the purring tremor (*frémissement cataire*). Let us see what has been said of them by the authors who have attempted their explanation.

Laennec thus commences the chapter which he has devoted to them: "The phenomena of which I am about to speak are so much the more remarkable, that among all those which mediate auscultation has made known, *they alone are not connected with any organic lesion to which they can be referred.*" (Op. cit., 2d edition, vol. ii, p. 421). And, in truth, in reading this chapter it is evident that Laennec regarded these different sounds as nervous phenomena only, the results of a particular modification of innervation. Their semeiotic value is therefore nothing, and it is impossible to draw from them any induction for the *localisation* of an affection of the heart.

Dance, who in his article *Auscultation*, of the new *Dictionnaire de Médicine*, has followed Laennec step by step, without formally explaining himself on the various sounds of which we are speaking, acknowledges that the stethoscopic signs in diseases of the heart, are far from having the certainty they possess in those of the lungs, and that they should be regarded so far only as they agree with the general symptoms which can never vary.

M. Bouillaud, whom we have seen adopting the opinion of M. Rouannet on the sounds of the heart, and like this physiologist, referring them to the play of the valves, regards the different abnormal sounds of which we are speaking, and especially the bellows sound, as pathognomonic of contractions of the orifices. The column of blood coming from the auricle, forced to traverse the contracted orifice more rapidly in order to gain the ventricle, roars and murmurs like a river which, after having flowed tranquilly and silently, suddenly meets with an obstacle. (Bouillaud, op. cit., vol. i, p. 173). But as he could not be ignorant that too many authentic facts were

opposed to the reference of the bellows sound, in all cases, to an organic contraction of the auriculo-ventricular orifices, he states further on, that he has likewise found the bellows-sound in cases of polypi of the heart; in those of fibrinous concretions formed in the ventricles, at a period evidently anterior to death; in cases of hypertrophy of the ventricles, the orifices not being contracted; and lastly, in a case of hemorrhage which subsequently produced the death of the patient (at the autopsy the heart was found healthy). In all these cases M. Bouillaud explains the bellows sound by an excessive friction (*frottement*) of the blood against the parietes of the orifices. Whatever be the value of this explanation, it nevertheless follows that M. Bouillaud has observed the bellows sound in too great a number of different cases, for it to be pathognomonic of any one in particular.

Finally, M. Pigeaux terminates his observations on these sounds by the following conclusions: "To recapitulate, we see, 1. that all the abnormal sounds, far from being other than the physiological, are but a simple modification of them, resulting from a friction increased either by the unusual rapidity of the blood, or by the asperities, and the rigidity of the valves which oppose its passage; 2. that the normal sounds are simply veiled by the pathological, but they exist notwithstanding; 3. that the abnormal sounds have no absolute value for denoting this or that alteration of the valves, and that they do not exist even when the valves are altered to the greatest extent."

In view of these facts, am I not authorised to say with M. Littré, author of the article *Cœur* (*Pathologie générale*) in the 2d edition of the *Dictionnaire de Médicine:* "As for the permanent rasping and bellows sounds, they denote in general an alteration of the orifices, either arterial or auriculo-ventricular. But what alteration, and of what orifices? There lies the difficulty. *It is clear that the explanations which will be given, will depend on the theory which may be adopted of the sounds of the heart.*"

The reflection of M. Littré is so just that, although published in 1834, it is still confirmed by the diversity in the new the-

ories which are incessantly brought forward for the explanation of these various sounds. Thus M. Beau has inserted in the *Archives générales de Médicine*, for 1838, an article entitled: *Recherches sur la cause des bruits anormaux des arteres, et application de ces recherches à l'étude de plusieurs maladies, et principalement de la chlorose*, in which, adopting the opinion already promulged by several physiologists, that the abnormal sounds of the arteries are owing to the thrill (*frémissement*) produced by the wave of blood on the arterial parietes by an exaggerated friction, he establishes as a principle, that the indispensable condition for the production of vibration in the tubes is the presence of a mass of liquid too great for the capacity of the vessel. On this principle he endeavors to prove that in all the diseases in which the abnormal arterial sounds have been observed, this disproportion exists between the calibre of the vessels and the quantity of blood which traverses them. This is easy in simple aneurisms, or in aneurismal varix. But applying his theory to other diseases in which the bellows or other sounds of the arteries have been heard, he draws the conclusion that, in *plethora*, there is a continued superabundance of blood capable of producing this disproportion; in *hypochondriasis* there is likewise a superabundance of blood, but temporary; that *chlorosis*, far from being a variety of anemia, is much rather produced by an exuberance of blood, differing from plethora in this particular, that the blood is deprived of a part of its nutritive qualities, and consequently discolored and more abundant in serum: an opinion, be it said in passing, rather contradictory to facts, and which M. Beau would find very difficult to prove.

Another writer, M. Delaharpe of Lausanne, treating the same subject in a memoir inserted in the *Archives génér. de Med.*, of August and September, 1838, under the title of *Nouvelles recherches sur le bruit de soufflet des artères*, has arrived, by experiments which it is useless to relate here, at this result: that the density of the blood plays a very great part in the production of the bellows sound of the arteries; that this sound is, in general and all things equal, the more intense as the

blood is less thick and its current more rapid; that this sound is produced in the artery by the current of liquid which traverses it, precisely in the same manner that the sonorous waves and vibrations arise in a column of air which traverses a tube.

Starting from this principle, the author denies that the bellows sound can be heard in cases of *plethora*, and thinks that there has been some error in the observations in which this sound has been noticed, and that the bellows sound was referable to some other affection. In cases of hypocondriasis, where the bellows sound has also been observed, it is owing to the diminution of the density of the blood, and to a great augmentation in the rapidity of the circulation. *Anemia* and *chlorosis* afford the most favorable circumstances for the production of the abnormal sounds of the arteries, by the thickening of the blood and the diminution of its quantity.

Thus then, two physicians investigating the same abnormal sound observed in the same diseases, but each explaining it after his own manner, have arrived at diametrically opposite results: M. Beau, that chlorosis is produced by an exuberance of blood which at the same time is much more serous than in the natural state; and M. Delaharpe, that in chlorosis there is thickening of the blood and diminution of its quantity. Which is to be credited? and who shall solve the problem?

Truly, he would have great trouble and extreme difficulty who should attempt to disentangle all these opinions for the purpose of deducing from them a certain theory, which universal reason might sanction, and which we might hope to see confirmed by experience. But I have not yet told all; and as if it were not enough that it is morally impossible, in the actual state of the science, to know to what organic lesion of the heart or of the vessels we should refer the abnormal sounds which occupy us, Dr. Graves, in a clinical review inserted in *The Dublin Journal of Medical and Chemical Sciences* (September 1834), cites a case of pneumonia which he observed with Dr. Marsh, and which presented an extremely loud bellows sound, which was heard over all the anterior

portion of the chest, but which could not be perceived in the carotids. This bellows sound followed exactly the progress of the disease, offered a greater intensity in proportion as hepatization advanced, and diminished as resolution was effected. The patient was very stout, had always enjoyed perfect health, and had none of the characters of a nervous temperament. Thus, if the observation of Drs. Graves and Marsh should be confirmed, the bellows sound will no longer belong specially to diseases of the heart. This is a new cause of uncertainty for him who seeks to found his diagnosis on the results furnished by auscultation; and still, I have not mentioned here the encephalic bellows sound observed by Dr. Fisher, of which I shall hereafter speak, for it is very evident that this could never be a source of error in diagnosis, at least in diseases of the heart, this sound evidently having its seat in the arteries of the brain, and never to be referred to the central organ of the circulation.

Thus far I have spoken of the bellows sound only; but by the unanimous consent of authors, the sawing, rasping and cooing (*roucoulement*) sounds are only modifications of the first, although they can not be constantly attributed to the same cause. The practitioner who has most distinguished himself by his laudable efforts to arrive at an interpretation of the organic affections of the heart by physical signs perceptible by means of the stethescope, and capable of aiding the diagnosis, M. Bouillaud, in striving to determine the modifications which produce this or that variety of the bellows sound, makes this remarkable avowal: "I greatly fear that the time has not yet arrived for the complete and definite solution of so delicate a question." (Op. cit., vol. i. page 166). He nevertheless proceeds to the explanation of these sounds, and says that the rasping, filing or sawing sounds seem to co-exist with an osseous or cretaceous induration, rather than with a fibrous or fibro-cartilaginous induration, coinciding with considerable diminution of the orifices and with strong and tumultuous movements of the heart. But this opinion, although supported by the great authority of M. Bouil-

laud, one of the authors who have most contributed in France to the advancement of the pathology of the heart, is nevertheless merely personal, and, although probable, can not be received as a demonstrated truth.

Still, if the number and the nature of these various abnormal sounds were well determined, and it were with them as with the pulmonary rhonchi, which, since Laennec described them, have always been found the same by all practitioners, perhaps we might hope soon to assign them an invariable semeiotic value. But such is not the case, and from time to time we see some observer point out some new sound not yet described, and of which no one again speaks afterwards. Such are: 1. the *musical sibilant râle* which Laennec occasionally observed, and which we find in musical characters in pages 424 and 426 of his vol. ii, (2d edit.), and of which I do not know that any one has since spoken; 2. the sound compared to the *hissing of a serpent* by Dr. Fischer, who observed it in a patient who thought he had some living thing in his breast; this sound was sufficiently loud to be heard at a distance; at the autopsy nothing remarkable was found, save a solid polypous concretion which filled the right ventricle, the auricle of the same side, and the vena cava, (*Journal de Hufeland*, 1821, vol. lii. page 3); 3. the sound analogous to the *croaking of a frog*, observed by Dr. Maas in a patient who was found on examination to have hypertrophy of the ventricle, with attenuation of the parietes of the right cavities and organic alteration of the walls of the aorta (*Journal de Hufeland*, 1826, vol. lxii. page 123); 4. and lastly, the barking sound (*bruit de hurlement*) described by Dr. Puchelt in 1833 (*Annales Cliniques de Heidelberg*, vol. ix. number 4). M. Puchelt had observed it in two patients who, at the autopsy, were found to have hypertrophy of the heart, accompanied with an aneurismal dilatation of the aorta.

This author regards his barking sound as the same which has been pointed out by Laennec, under the name of the *musical sibilant râle;* by M. Fischer, under the name of *hissing of a serpent;* and by M. Maas, under that of *croaking of a*

frog. But these different sounds have been compared by the several observers to things too opposite for us to believe that they are the same, and I do not see how Laennec could have musically written the hissing of a serpent or the croaking of a frog. If, however, these sounds are evidently not the same, may they at least be referred to the same lesion? M. Puchelt does not hesitate to do so, and he places the cause in the aneurismal dilatation of the aorta at its origin, and in the organic alteration of its parietes. But in publishing this opinion, M. Puchelt did not reflect, that in the results of the autopsies mentioned by M M. Fischer and Maas nothing similar is noted.

But let us suppose that demonstrated which we have just proved not to be so: let us suppose that each of these abnormal sounds has a determinate and invariable value; how shall we distinguish in which side of the heart it is produced, and consequently which is the diseased side? Laennec has laid it down as an almost invariable rule, that the movements of the left cavities of the heart are principally felt between the cartilages of the fourth and seventh left sternal ribs, and those of the right cavities under the inferior part of the sternum, "so that in case of disease of one side of the heart, the analysis of the pulsations of this viscus, affords results altogether different in the two points." (Op. cit., vol. ii. p. 386). Most authors who have written on this subject, have adopted the opinion of Laennec; and it is morever on this distinction between the points where the beatings of the two ventricles are heard, that M. Bouillaud founds in part his differential diagnosis between hypertrophy of the right ventricle and that of the left. (Bouillaud, vol. ii. p. 447.) But another author whose name is of no little weight in auscultation, Dr. Spittal, is of a contrary opinion, and thinks that, in most cases, we cannot arrive at a precise diagnosis; and that those in which the affected side has been accurately determined are only rare exceptions.

In an article entitled: "*Remarks on the following question: Is it possible, in a case where there exists a dilatation or a hypertrophy of one side only of the heart, to discover by auscul-*

tation which is the diseased side?" and inserted in the *Edinb. Med. and Surg. Jour.* (January, 1834), this physician boldly decides in the negative, and in support of this opinion refers to a case in which all the symptoms denoted that the left side of the heart was diseased, and yet, at the autopsy, this side was found quite healthy; but as a compensation, the right side presented a considerable dilatation and hypertrophy. And as a single fact was not sufficient to prove this opinion, he has had the patience to collect eighty cases of diseases of the heart, from the works of Laennec, Bertin, Bouillaud and Dr. Hope; and has found the indication of the diseased side in eighteen cases only, among which but four furnished on dissection a confirmation of the diagnosis.

The conclusion of Dr. Spittal is severe; I must even say that it appears to me exaggerated. I think that an excellent means of ascertaining the diseased side of the heart, is that which M. Littré has pointed out in an article inserted in the *Gazette Médicale de Paris* (1834, p. 499). It consists in listening to the sounds of the heart, not only at the precordial region, but also over the whole chest. We shall then find that if on one side we hear the abnormal sound, on the other we most frequently hear the regular double sound without alteration. The diseased side is therefore denoted by the side of the chest where the abnormal sound is heard. If this sound be heard alike on the right and left, we must conclude that both sides of the heart are equally altered. There would probably not be an exception to this rule save in the very rare cases in which the abnormal sounds of the heart are so loud, that there is no need of the stethescope to hear them. The precepts of M. Littré are very wise, founded on observation; but still this is only an element of progress, and not a real improvement to be noticed in the pathological history of the heart.

Lastly, auscultation does not furnish more certain and more invariable data for the diagnosis of diseases of the aorta. Thus Dr. Hope had laid down a rule, for a long time admitted without exception, that, when the bellows sound is louder

over the course of the descending aorta than opposite the valves, and when at the same time it is superficial and accompanied by a hissing sound, it is always the result of disease of this vessel; but Dr. Henderson has reported in the *The Edinburgh Medical and Surgical Journal* (January, 1835) two observations in which a bellows sound, offering all the characters that we have described, coincided with a dilatation of all the cavities of the heart, the parenchyma of which was at the same time hypertrophied: *the aorta was perfectly healthy.* Continuing his researches on this subject, the same author, in an article inserted in the same Journal in 1835, proves that the sounds furnished by aneurisms of the arch of the aorta, vary according to the form, the presence or absence of coagula in the interior of the aneurisms, and according to the extent of the opening by which they communicate with the aorta: aneurisms obstructed and filled with coagula, furnish by themselves no sound, and transmit but one which comes from the heart. When the aneurism produces one which is peculiar to it, it is a rasping sound, that is to say, in other terms, that these sounds yielded by aneurisms of the aorta are susceptible of being modified by so many causes wholly inappreciable to the observer, that it is impossible to rely solely upon them in establishing a diagnosis of the diseases which we are studying.

I might, perhaps, here take leave of this discussion respecting the uncertainty of the stethescopic signs of diseases of the heart, thinking its source had been sufficiently demonstrated; but I should not think my task finished, if I did not succinctly touch upon the principal diseases of which the heart and its appendages may be the seat, to find what their diagnosis has gained by the discovery of Laennec.

Pericarditis, acute and chronic. Hydro-Pericardium.

According to all authors pericarditis has ever been one of the most difficult diseases to recognise. "I have sometimes seen pericarditis *guessed*", says Laennec, "and I have occa-

sionally *guessed* it myself; for I do not think we can employ the word *recognise*, when there are no certain signs, and it happens that we are as often deceived as we guess aright." (Op. cit., vol. ii, page 660.) Corvisart, who likewise admits the extreme difficulty of the diagnosis of this disease, gives as a reason for it, that it runs its course too rapidly to allow the determination of its character; that it is almost always complicated with inflammation of the neighboring organs whose symptoms conceal its own, or which at least mingling with them, form a multiplicity of phenomena in the midst of which it is very difficult to know which is the principal disease. (Corvisart, *Essai sur les maladies organiques du coeur*, p. 5). However it may be with this explanation, it is certain that the præcordial pain, which is ordinarily described as lancinating, pungent, and very severe, is sometimes wanting or is so slight that, to excite it, percussion of the thorax, or pressure on the epigastrium must be resorted to; either it is masked by some other rheumatic, pleuritic or articular pain, or the patient is entirely free from suffering. The augmentation in the force of the pulsations of the heart, their irregularity, their intermittence, the dyspnœa, are also signs of pericarditis which are so frequently wanting that Laennec declares: "That a certain degree only of confidence is to be placed in these signs, even when they are all united; for not only may pericarditis exist without them, but the whole may likewise be present without there being pericarditis." (Op. cit., vol. ii, p. 663). Will auscultation remove this difficulty in the diagnosis? Let us again hear Laennec: "I must admit that mediate auscultation does not give much more certain signs of pericarditis, than the study of the general and local symptoms." (Op. cit., vol. ii, p. 662). And indeed, he indicates no stethoscopic sign belonging specially to this disease. M. Collin however in his thesis entitled: "*Des diverses méthodes d'exploration de la poitrine, et de leur application au diagnostic de ses maladies*" (Paris, 1823), had before spoken of the sound like *new leather* (*bruit de cuir neuf*) as properly belonging to acute pericarditis. This sound has

since been observed by M. Bouillaud, who compares it to the grating sound (*bruit de frottement*) which often accompanies the first stage of pleurisy, and which Laennec himself has indicated under the name of *murmur ascensionis et descensionis*. Several English observers have pointed it out, among others Drs. Hope, Stokes and Watson. The latter, in an article published in the Lancet of 1836, has asserted, that whenever during the continuance of rheumatism a grating sound was heard in the precordial region, we might be sure that an acute pericarditis was being developed; that if this sound continued until the death of the patient, the pericardium would be found covered with a rough, non-adherent layer of lymph, at least in the greater part of its extent; and that if it ceased, and did not reappear, it was owing to the pericardium having contracted adhesions in the greater part of its surface by which the friction was prevented. The opinion of Dr. Watson, a little exaggerated perhaps, on the constant formation of adhesions between the laminæ of the pericardium when an effusion has taken place there, has been since contradicted by Dr. Root, who thinks that, when the grating sound has revealed an effusion in the pericardium, it is not impossible that the latter should return to nearly its normal state, save however the formation of those white blotches of variable size, so often observed on the heart, and which he regards as the signs of an inflammation of the pericardium terminated by a partial though not entire absorption. It results therefore from the observation of these different authors, that the grating sound denotes in general, acute pericarditis, with a slight effusion; but unfortunately it does not constantly exist.

It would appear, from these writers, that the same sound is heard in the case of those milky patches of the heart which so often follow pericarditis. But the adhesion of the pericardium to the heart, another pretty common result of this disease, is not disclosed by any symptoms, and appears to be much more innocent in itself, than Corvisart thought. As to its being a frequent cause of dilatation of the ventricles, as asserted by M. Beau in a memoir published in the *Archives*

de Médicine, April, 1836, no one certainly will believe it, if he has reflected ever so little that the facility with which considerable effusions are formed in the pericardium, is the best proof of the extensibility of that membrane, a physical property diametrically opposed to that which would be indispensable for the production of the effect indicated by M. Beau.

Chronic pericarditis is not of easier diagnosis than the acute, but its most ordinary effect is an accumulation of pus in the serous envelope of the heart. Now, in this respect, its diagnosis is confounded with that of hydro-pericardium, although, in the eyes of the pathologist, there is a vast difference between an accumulation of pus, and one of limpid serum in a serous cavity, and it would be impossible for him to attribute both to the action of the same cause.

I shall not stop to discuss the quantity of serum necessary for one to say there is hydro-pericardium: it is not my object. I hasten to the pathognomonic signs indicated by authors for this disease; the principal are: the sensation of an enormous weight in the precordial region (Lancisi); the sensation of the heart swimming in a great quantity of water (Reimann and Saxonia); the inability of the patient to lie on the right side, without being on the point of suffocation: all signs which, according to Morgagni, are scarcely worthy of being put on the list even of equivocal signs of this affection. As much might almost be said of those which Corvisart indicates in this passage: "In patients affected with hydro-pericardium, the countenance is habitually violet, the lips black and livid; they experience a painful anxiety, an irksome weight in the precordial region, a difficulty of respiration which threatens suffocation whenever an attempt is made to assume a horizontal posture; syncope frequently occurs; etc." (Op. cit., p. 41). Sénac has seen in the intervals of the third, fourth, and fifth ribs, the undulations of the effused liquid. (Sénac, op. cit., vol. ii, p. 364). Corvisart says, he has recognised them by the touch, but only in one case, and yet he dares not affirm that he really felt the wave of the effused liquid, and not an undulation produced by the pulsations of the heart: "But I

feel assured," he adds, "that if it be thus, the particular character of these pulsations is very perceptible." (Op. cit., p. 42). M. Bouillaud says, with reference to this passage, that he has once had occasion to make an analogous remark. He thought, at first, he had ascertained the existence of fluctuation in the precordial region; but an attentive examination soon convinced him that the phenomenon taken for fluctuation was nothing else than the contraction of the heart, which had been pushed from its accustomed place, and applied as it were against the thoracic parietes by an enormous tumor situated in the left side of the chest. (Bouillaud, op. cit., vol. ii. p. 336).

But Corvisart has drawn from percussion of the thorax (a method which he brought to such perfection, that more honor has accrued to him, than even to Avenbrugger its inventor), signs much more positive, than those just enumerated: "When percussion of the chest is practised," he says, "whether the patient be sitting or placed horizontally in his bed, the sound yielded by this cavity is obscure, and indeed absent anteriorly and on the left, over an extent proportioned to the dilatation which the liquid causes in the pericardium, In some cases the left side of the chest is more elevated, more rounded and more prominent than the right, etc." (op, cit., p. 4). Dulness of the precordial region and prominence (*voussure*) of the same, are the new signs first indicated by Corvisart, and they have since been noticed by all observers. They are really of great value, but are yet insufficient to show incontestably that there is hydro-pericardium. Who shall prove, indeed, that this flatness of the precordial region is not owing to an augmentation of the volume of the heart? . . . . . Corvisart afterwards adds, that a sign which appears to him to merit more confidence than all others, is the sensation of the pulsations of the heart in different points of a pretty extensive circle, which would not take place if the heart was retained by the pericardium as in its ordinary state. (Op. cit., p. 43). Here Corvisart no longer agrees with other observers, who appear unanimous in saying that in cases of hydro-pericardium, the

hand applied on the precordial region does not feel the pulsations of the heart.

Will auscultation finally decide this question? Will it solve the problem of the diagnosis of hydro-pericardium? Let us hear Laennec: "In these cases the stethescope will doubtless aid in establishing a diagnosis, but I cannot say what signs it will furnish, because I have not had sufficient opportunities of observing idiopathic hydro-pericardium." (Op. cit., vol. ii, p. 670). Bouillaud says that when the fluid in hydro-pericardium is abundant, "the sounds of the heart are distant, obscure and resemble somewhat those of the fœtal heart." And in support of his opinion he cites a passage from a case of hydro-pericardium communicated to him by M. Casimir Broussais, in which, "only feeble and obscure contractions were heard, and the more obscure as the distance was greater from the region corresponding to the base of the heart; so that in this point the two contractions were distinct, whilst at the extremities of the region occupied by the flat sound a sort of murmur only was perceived, somewhat analogous to that heard when a shell is held to the ear." (Bouillaud, op. cit., vol. ii, p. 338).

Theory long before indicated this obscurity in the sounds of the heart, which to reach the ear traverse the fluid poured out into the pericardium; but if we reflect how the intensity of these sounds varies in different persons, according to the greater or less embonpoint or other similar cause, we shall be easily convinced how transient this sign is, and consequently of the little value it possesses as it respects diagnosis.

Carditis. Endo-Carditis.

Carditis, or general inflammation of the muscular tissue of the heart, has most often been confounded with pericarditis or with inflammation of the internal serous lining of the organ, which we shall speak of under the name of endo-carditis. This affection, if it really exists separately, and confined to the fleshy tissue of the heart, has as yet manifested itself only by

the formation of abscesses in this tissue, which have never been revealed except by dissection. No attempt has yet been made to facilitate their diagnosis by auscultation: I have nothing more to say of them.

Not so with endo-carditis. This affection, of which Corvisart says not a word, and which was regarded as very rare by Laennec, has been carefully studied by M. Bouillaud, who sees in it the cause of all the fibrous, osseous or even calcareous degenerations, which may be presented by the valves of the heart or of the great vessels. I shall not discuss this opinion, connected as it is with the doctrines of the physiological school, which, as is well known, is disposed to consider inflammation the sole cause of every possible organic alteration. The fallacy of this doctrine was long ago shown; a proper estimate has long since been placed on these erroneous and exclusive opinions, which go to strike the changes and decomposition of the fluids from among the causes of abnormal productions in our economy, to refer every thing to an unnatural afflux of blood, whilst this afflux often does not take place, or at least is only momentary, and sometimes is only the consequence of an already advanced stage of organic alteration, instead of being its cause.

Since M. Bouillaud, several authors have treated of endo-carditis, among others M. Littré, in the second edition of the *Dictionnaire de Médecine;* Dr. Cazaneuve, of the military hospital of Val-de-Grâce, who has made it the subject of his inaugural thesis, inserted in the *Gazette Médicale* of 1836, (p. 401); and Dr. Watson, in an article on rheumatic carditis, published in the *Lancet* in 1837, etc. The history of this disease being somewhat new, auscultation has been put under contribution like the other modes of investigation, to establish a diagnosis; but, as in other diseases of the heart, it affords only hypothetical data. The signs indicated by M. Bouillaud are: prominence (*voussure*) of the præcordial region, where there is pericarditis at the same time; vibration of this region from the violence of the pulsations of the heart, which assume the character of palpitations; the sensation of a vibratory

thrill (*frémissment*), more or less marked in the same region; smallness and slight developement of the pulse, coinciding with the violent contractions of the ventricle; anxiety, jactitation, fainting and syncope.

The region of the heart, in its entire extent, gives a flat sound on percussion. This flatness, coincides with the violent pulsations of the heart, is thereby distinguished from that which accompanies hydro-pericardium, in which these pulsations are not felt.

After this statement of symptoms, which is sufficiently exact, and conformable to theory, what shall we demand of auscultation? some constant and invariable physical sign, doubtless, which would enable us to recognise the disease in all cases where the general symptoms just enumerated should be wholly or in part wanting? Alas! as to any pathognomonic sign, we obtain the bellows sound only, and sometimes, according to M. Bouillaud, a metallic tinkling synchronous with the systole of the ventricle, and which M. Cazaneuve, who in his thesis has closely followed Bouillaud, attributes to the shock of the heart against the parietes of the thorax. In vain does Professor Bouillaud say, that this bellows sound is stronger as the pulsations of the heart are more violent and precipitate; in vain does Dr. Watson affirm that every time a deep blowing sound supervenes in the course of a rheumatism, it constantly denotes an inflammation of the internal membrane of the heart, particularly of that portion which covers the valves; I shall still refuse to consider the bellows sound a pathognomonic sign of endo-carditis.

Let it be remembered, now, that by the unanimous consent of pathologists, the bellows sound nearly always coincides with contraction of the orifices of the heart. Laennec had remarked this, although he added, that the cases in which the bellows-sound was observed without there being any organic alteration of the heart whatever, were too frequent to make the production of the sound depend essentially on this contraction. (Laennec, op. cit., vol. ii, p. 441). M. Andral, in speaking of the bellows-sound, says, it is very certain that

in a great number of cases its existence coincides with that of an obstacle to the free course of the blood through the different orifices of the heart. He even goes so far as to say that, "according to the place where this sound is heard, and the time of the contraction which accompanies it, we may even assign the precise seat of the obstacle." (*Clinique Médicale*, 2d edition, p. 161). But he afterwards makes the same reservation as Laennec for the numerous cases where the bellows sound has existed in healthy hearts, or has been wanting in hearts presenting the alteration in question. We have seen M. Bouillaud profess the same opinion, and compare this sound to the roaring of a river which rushes forward against some obstacle; but if the bellows sound essentially indicates a contraction of the orifices of the heart, what does it indicate in endo-carditis?

What, indeed, are the anatomical characters of endo-carditis? "Redness, and sometimes softening of the membrane, frequently a *considerable thickening*, which exists very evidently only on the valves, where the membrane is, as it were, doubled and strengthened by a fibrous tissue." (Bouillaud, op. cit., vol. ii, p. 174). But is this *considerable thickening* sufficient to hinder the circulation of the blood through the orifices? this will be doubted, if we reflect that, by M. Bouillaud's own confession, the vegetations of the internal membrane of the heart and of the valves, are revealed by no sign, no sound perceptible with the stethoscope, and that they are, according to the forcible expression of this author, *undiagnosticable*. Now, ought they not to hinder the circulation an hundred times more than simple thickening of the endo-cardium, however *considerable* it may be?

But this is not all. M. Littré has nearly demonstrated, in a memoir inserted in the *Gazette Médicale de Paris* (under the title of *Recherches sur l' insuffisance des valvules auriculo-ventriculaires et des valvules sygmoïdes de l' aorte*, that the bellows sound is likewise an essential sign of this insufficiency, or, to explain myself better, of the regurgitation of the blood through the orifices, during the contraction of the ven-

tricles, when the mitral and tricuspid valves are defective: and during their dilatation, when the semilunar valves are diseased. This insufficiency of the valves may arise, either from ulceration, ossification, or from the orifice to which they are attached being so much dilated, that they can no longer close it completely.

Thus, then, the bellows sound is at the same time a pathognomonic sign of contraction of the orifices, and of their dilatation, which necessarily renders the valves insufficient: and the only character which is given to distinguish the former from the latter, is that: 1. in contractions of the auriculo-ventricular orifices, the bellows sound replaces the second sound, or that accompanying the dilatation of the ventricle; 2. in the case of insufficiency of the valves of these same orifices, the bellows sound is heard at the moment of the contraction of the ventricle, and consequently replaces the first sound. But when the insufficiency of the semilunar valves will permit the blood to flow back from the aorta, during the dilatation of the ventricles, the bellows sound will still replace the second sound: how then are we to distinguish this case from that of contraction of the auriculo-ventricular orifice? M. Littré says that, in the case of insufficiency of the semilunar valves, the bellows sound is prolonged into the aorta (Littré, loc. cit.): it is really expected, then, that we should always establish with mathematical precision the limits of the space where the bellows sound is heard! that we should discover constantly whether this sound does not extend beyond the ventricle, or that it is slightly prolonged in the arteries! Is it not enough to say which of the sounds of the heart is replaced by the bellows sound, and to seize just the moment in which it is heard, in a space of time so short as that of the double pulsation?

To recapitulate, an abnormal sound which is found to be the essential sign of three affections, so different as simple endo-carditis, contraction of the cardiac orifices, and insufficiency of the valves, which is itself often an effect of the dilatation of these orifices, is not the pathognomonic sign of either:

I think that I have sufficiently proved it. I may therefore rightfully conclude that auscultation furnishes no reasonable and sufficient basis on which to establish the diagnosis of endocarditis.

Hypertrophy. Dilatation of the Heart.

These two pathological states, altogether distinct, and perfectly capable of existing separately, have always been confounded under the name of aneurisms. Corvisart himself did not perceive the essential differences which distinguish them from each other, and treats only of aneurisms, which he divides into *active* and *passive*, accordingly as they are accompanied by hypertrophy or attenuation of the parietes of the heart.

Having to treat of these diseases with respect to their diagnosis only, I proceed at once to the symptoms of hypertrophy. M. Bouillaud thus briefly analyses them: "Permanent augmentation of the force and extent of the heart's pulsations, and consequent augmentation of the double normal sound which accompanies them: . . . . increased extent of the dulness in the præcordial region, and sometimes a marked projection, a manifest prominence (*voussure*) of this region." (Op. cit., vol. ii; p. 440). These signs, however, are not applicable to all cases of hypertrophy. Thus, the flat sound of the præcordial region is not obtained over a space greater than in the healthy state, except in *excentric* hypertrophy, the sonorousness of this region remaining the same in cases where the hypertrophy is developed at the expense of the cavity of the heart. It is likewise in the first only, that the pulsations of the heart are increased so as to be perceived by the eye, to raise the hand applied on the præcordial region, and sometimes to shake the whole body. I say nothing of the pulse, because it is subject to so many variations that it would be impossible to draw the least sign from it. Neither do I speak of the dyspnœa, of the disposition to hemorrhagies and dropsies, rational signs which do not belong to hypertrophy more than to the other diseases of the heart. I hasten to take up the results obtained by auscultation.

The stethoscopic signs vary according to the nature of the hypertrophy. If it be concentric, the sounds of the heart are dull, obscure, as it were smothered; but if it be excentric, these sounds are louder, more sonorous, clearer, extending over a greater portion of the chest, and often even to the posterior part of this cavity. In this case a metallic tinkling or clacking is frequently heard in the anterior part of the chest, resulting from the shock of the heart against the posterior part of the *sternum*. (Bouillaud, op. cit., vol. ii, p. 442). To these signs M. Chomel adds the bellows sound: "for," says he, "it has been wrongly contended that the bellows sound belongs exclusively to the contraction of the valves; a great number of autopsies has proved to me that hypertrophy without contraction may also produce it." (Art. *Hypertrophie et Dilatation du cœur*, 2d edit. of the Dict. de Méd., or Répert. génér. des Scienc. Médic., vol. vii, p. 295).

Is it possible by means of auscultation to distinguish the diseased from the healthy ventricle? I shall not recur to this question already sufficiently discussed while examining the opinion of Dr. Spittal who, as we have seen, disagrees with Laennec on this point: I will only add that the stethoscopic signs are sufficient for this, in so far only as they accord with the general symptoms of reaction, which are known to differ greatly in the two cases, and to affect chiefly the encephalon in hypertrophy of the left ventricle, and the organs of respiration in that of the right.

As to the dilatation of the cavities of the heart, if it be accompanied, as it most commonly is, by hypertrophy of the parietes, the signs are those which I have just enumerated, save that the dulness on percussion extends over a larger surface. But if the dilatation be complicated with attenuation of the walls of the heart (an extremely rare case), it will likewise be recognised by the dulness of the præcordial region, and the softness of the heart's pulsations, which are obtuse and as if muffled, and lastly by the general signs which indicate a general languor of the circulation. By means of the diagnosis which I have just indicated, hypertrophy and dila-

tation of the heart may in general be recognised pretty well; but it must be confessed that, among all these symptoms, those furnished by auscultation are far from having that marked preponderance which characterises them in affections of the lungs. In these the stethoscopic signs are every thing, the general symptoms are but secondary. In diseases of the heart, on the contrary, the stethoscopic signs are valuable so far only as they perfectly agree with the general symptoms. But, in the latter case, this unanimous agreement of all the general and local symptoms does not always prevent error. What more common, indeed, than to observe purely nervous palpitations, perfectly simulating hypertrophy and dilatation of the heart, in patients who have this organ entirely sound! Will the application of the stethoscope, however, serve to distinguish this purely nervous state from the superexcitation of the heart produced by hypertrophy? Not at all! By auscultation we shall in most cases hear that unfortunate bellows sound, the constant attendant of every derangement of the circulatory function, even the slightest, and which is destined to be a stumbling-block to all who shall try to *localise* the diseases of the heart by means of the stethoscope. Many nervous or asthenic affections, as hypochondriasis, nostalgia, chlorosis and scurvy, are known to be frequently accompanied by palpitations which simulate to a certain degree the organic affections of the heart, and which are always attended by the bellows sound. I have already had occasion to say to what diametrically opposite conclusions MM. Beau and Delaharpe had arrived, in the explanation of these facts, by starting from contradictory opinions on the first cause of the bellows sound: I shall not revert to them.

There are certainly means of distinguishing in time palpitations purely nervous from those connected with an organic affection. Thus, the former have but a temporary duration: their course is intermittent; they may finally cease; they do not cause dropsy: the sonorousness of the cardiac region is not modified. But no differential symptom is furnished by auscultation; this is a fact which I wished particularly to estab-

lish: and in these diseases, as in the preceding ones, it seems evident to me, that this method of investigation has not added much to what the science already possessed. It is then with reason that M. Chomel has penned these lines, truly remarkable as coming from him: "What has already been said of the phenomena peculiar to hypertrophy and dilatation of the heart, will in general serve to distinguish these two affections from each other. It is, however, necessary to be aware, that at all periods of these diseases, and especially in their advanced stage, it is often very difficult and even impossible to distinguish them; and that, in many cases, the physician who does not wish to hazard his judgment, should limit himself to the announcement of a disease of the heart, *without determining either its special seat or even its nature.*" (Chomel, loc. cit., 2d edition of the *Dict. de Med.*, or *Repert. génér. des Scienc. Medic.*, vol. viii, p. 299).

To be faithful to the terms of the programme, I should now take up the signs furnished by auscultation for the diagnosis of diseases of the great vessels, and particularly of aneurisms of the arch of the aorta. But Laennec himself despaired of arriving at any satisfactory result on this point. "Among all the severe lesions of the thoracic organs," says he, "three only remain without a constant pathognomonic sign to the physician expert in auscultation and percussion, to wit: aneurism of the aorta, pericarditis, and concretions of blood in the heart previous to death." (Op. cit., vol. ii., p. 737). We are indebted to M. Bouillaud for the indication of clear, simple or double pulsations, according to the relations of the aneurismal tumor, and distinct from those of the heart, which are perceived by the ear in a circumscribed space behind the sternum or the cartilages of the right false ribs. (Bouillaud, *Diagnostic des anévrisms de l'aorte*, theses de Paris, No. 146, 23d August, 1823). This sign, which is really valuable, requires to be fortified by the concordance of other symptoms, which it does not enter into my plan to develope here.

I stop here in the inquiry as to what has been effected by auscultation applied to diseases of the heart. I think that

I have amply shown what I said in the commencement of this chapter, that its results, so far as they concern the diagnosis of those diseases, are as doubtful and as little capable of guarding against error, as they are certain and endowed with an immense scientific importance in diseases of the lungs. In the latter, the stethoscopic signs alone reveal the disease, even if it be in a latent state, and have not yet excited any general symptom. In diseases of the heart, they give rise to probabilities by no means sufficient to throw light upon the nature of the malady, and still less upon its precise seat.

Does it follow that we must ever despair of this method applied to the investigation of these diseases? It is not in an age like ours that such a scientific heresy would be uttered. By no means; auscultation of diseases of the heart is not without a future; and we must not despair of seeing it some day effect for them what it has already done for diseases of the lungs. Since Laennec, auscultation of the heart has made undoubted progress; but this progress has chiefly consisted in correcting the errors which escaped his sagacity and penetration. Let observers, therefore, continue their labors, and perhaps success will not be long in crowning their efforts! but let them avoid the obstacles which hitherto have baffled those of their predecessors. These obstacles are found in the too exclusive ideas respecting the cause of the normal sounds of the heart, the only source from which an explanation of its abnormal sounds can be drawn. All the theories which I have developed on the cause of the sounds of the heart, are, in fact, too absolute to be in all respects true. Why should not the contraction of the fibres and of the fleshy columns of the heart, the play of the valves, the friction of the columns of blood against each other or against the parietes of the cavities, and even the shock of the heart against the sternum, equally contribute to the production of the double pulsation? This opinion is not peculiar to me; it is entertained by several physicians, among others by Professor Andral, who, in the additions which he has made to Laennec's work, does not seem far from admitting that, "among the causes to which each

author has exclusively attributed these sounds, there is none which may not have its part in their production, but that no single one is sufficient to give rise to them."

I think that it is only from inquiries directed in this way, and made after this mixed and truly eclectic theory, that we can hope for favorable results, which would give to auscultation, in the pathology of the heart, the rank which it occupies in that of the lungs. Time alone will decide whether my hopes are well founded.*

CHAPTER V.—OF THE NERVOUS DISEASES OF THE CHEST: ASTHMA; ANGINA PECTORIS.

It is not without design that I have reserved these two diseases to be treated of last, as what I shall say concerning them will be but the corollary from all that precedes. Indeed it is only after having firmly established the diagnosis of the acute and chronic diseases of the lungs and heart, that these organs should be interrogated again, to discover by the aid of the stethoscope some organic alteration which might satisfactorily explain those attacks of violent dyspnœa or of præcor-

* It is very evident from the whole tenor of this chapter, that the author was unacquainted with the more recent inquiries into the action and sounds of the heart, and with the improvements consequently effected in its pathology. In no other way can we account for the low estimate he places on the value of auscultation applied to the diseases of that organ.

Among these researches, those of Dr. Hope stand pre-eminent. His opinion respecting the sounds of the heart, published in 1832, which is quoted by Dr. Peyraud on page 92, was greatly modified by farther experiments made in 1834–5; these experiments were repeated and confirmed by the London committees of the British Association for 1838–9 and 1839–40, and by Drs. Pennock and Moore of Philadelphia, in 1839: so that now the causes of the cardiac sounds are pretty well understood. It was our design to notice this matter at some length, but want of space forbids. We would refer the reader, who desires further information, to the last edition of Dr. Hope's Treatise on the Diseases of the Heart and Great Vessels, which is now republishing in Prof. Dunglison's *Medical Library and Intelligencer*, with notes and details of recent experiments by Dr. Pennock of Philadelphia. He will not fail to perceive that the *future* of Dr. Peyraud has become the *present*, and that the diagnosis of cardiac affections has attained a certainty which falls little short of that previously acquired in those of the chest.—Jun. Ed.

dial pain, whose nature the ancients absolved themselves from inquiring into by accusing the nervous system.

Not but that a great number of authors have made laudable efforts to find in the dead body the causes of these affections, so inexplicable even at the present day: and if we refer to their assertions, we shall see that there is no lesion of the thorax or of the organs it contains, which may not produce attacks of intermittent dyspnœa. Consolidation of the ribs by ossification of the cartilages which unite them to the vertebral column and to the sternum; ossification of the diaphragm; diaphragmatic hernia; rickets and deformity of the thorax; chronic pleurisy, or else adhesions between the two pleuræ; sojourn of certain foreign bodies in the cavity of the respiratory apparatus; developement of polypous productions and of more or less voluminous vegetations in those same cavities; œdema of the glottis: chronic pulmonary catarrh with contraction of the bronchiæ; compression of the bronchiæ by some neighboring tumor; bony or osteo-petrous concretions in the mediastinum, or even in the pulmonary parenchyma; pericarditis; hydro-pericardium; ossification of the valves of the heart, of the coronary arteries, etc. etc.; what is there that has not been cited as the exciting cause of asthma by some author, supporting himself on some fact? As to angina pectoris, the authors who have treated of it appear to attribute it to organic diseases of the heart and of the great vessels, such as ossification of the coronary arteries, hypertrophy of the ventricles, ossification of the valves, osseous degenerations of the walls of the aorta, or the aneurismal dilatation of that vessel; all alterations which we have seen mentioned for the most part, as causes of asthma; so true is it that these alterations are not every thing in the production of the singular phenomena whose assemblage constitutes these two diseases, if, indeed, these lesions are not the mere effect of them, instead of contributing any thing to their formation!

This opinion, that the organic alterations are the effect, and not the cause of asthma, is not new: many practitioners have adopted it, despairing of explaining by those lesions the inter-

mittent character of the diseases attributed to them. It has been sustained by M. Amédeé Lefèvre, among others, in a memoir entitled: *Recherches médicales sur la nature, et le traitement de la maladie connue sous le nom d' asthme*, an essay which was crowned by the Toulouse Medical Society, at its sitting of 7th May, 1835, and inserted in the *Journal hebdomadaire de Médecine*. But, at the same time that he excludes organic alterations of the heart and great vessels as causes producing asthma, he likewise rejects the opinion which regards this disease as a neurosis, and he thinks that asthma is always owing to a spasmodic contraction of the bronchiæ, which may be produced by all the causes which act, either directly or sympathetically, upon the pulmonary mucous membrane. According to him, the cause of asthma always acts on this membrane, and the affection of the muscular fibres of the bronchiæ is consecutive only.

These ideas resemble those of Dr. Godefroy of Mayence, who, in a memoir on *Asthma essentiel considéré comme névrose des bronches*, attributes this malady to a spasmodic contraction of the muscles of Reisseissen, whence results the diminution of the capacity of the bronchial tubes. But the discussion of the greater or less probability of these theories, is altogether foreign to my object, which is simply to find whether auscultation cannot, in certain cases, aid in discovering the organic lesion which is the occasional, if not essential cause of the attacks of intermittent dyspnœa.

There is a disease which had always escaped the attention of practitioners, because its anatomical characters consist in a mere exaggeration of the natural state of the lungs, and its symptoms are confined to a greater or less dyspnœa, varying in intensity according to a multitude of circumstances. This disease, which Laennec first pointed out, as frequently constituting the essential anatomical character of a great many cases of asthma, till then regarded as nervous, is *pulmonary emphysema*. It is known to consist in the distention of the pulmonary vesicles by atmospheric air, which is accumulated there in much greater quantity than in the healthy state

Sometimes the disease stops here, and is only a permanent, excessive, and preternatural distension of the pulmonary vesicles, and then the emphysema is said to be *vesicular;* at other times the distention increasing, the cells burst, and the air, which in the first case was contained in its natural passages, is extravasated in the surrounding cellular tissue: the emphysema then takes the name of *interlobular.* I shall not examine here, whether, as Laennec thought, the pulmonary tissue is sound, apart from this dilatation; or whether there is at the same time hypertrophy of the parietes of the air cells, as M. Louis thinks, in his article *Emphysème des poumons,* (*Dict. de Med.*, 2nd edit.) I pass on to the signs which enable us to distinguish this disease.

The most constant symptom of pulmonary emphysema is a dyspnœa more or less severe but continual, subject, however, to exacerbations which perfectly simulate the attacks of asthma, I should rather say, which constitute asthma itself. Indeed, like the attacks of asthma, these exacerbations supervene without any known cause, and their duration varies with the cause that produces them; very rare at the onset of the disease, they become more frequent and severe the longer the disease continues, and are frequently accompanied by palpitations, which increase the embarrassment of the practitioner, and sometimes would lead him into an erroneous diagnosis, if Laennec had not indicated certain signs for this affection, all drawn from percussion and auscultation.

The side of the chest, the seat of pulmonary emphysema, yields on percussion a very clear sound; and yet auscultation indicates, over most of the same side, only a complete absence of the respiratory murmur, with a slight sibilant râle from time to time, mingled with that mucous râle which Laennec has compared to the clacking of a valve, and which he regards as the index of the displacement of pearly sputa. This absence of the respiratory murmur will not be mistaken for that produced by a pleuritic effusion, because percussion would then yield a flat sound, instead of the very clear sound which I have named. Neither will it be confounded with that which characterises pneumo-thorax, because in this case, the respi-

ratory murmur extends along the vertebral column, the point where the lung retires, compressed by the air effused in the pleura: whilst in emphysema, the respiratory murmur ceases to be heard in all the emphysematous zone, towards the root, as well as the periphery of the lung. But there are cases in which a *dry crepitant rattle with large bubbles* is heard, which M. Louis has indicated under the name of *sub-crepitant râle*. Laennec had regarded it as indicative of *interlobular* emphysema. M. Louis justly considers it as depending on the pulmonary catarrh which always accompanies emphysema. As to the *ascending* and *descending* rubbing or grating sound (*bruit de frottement*), likewise pointed out by Laennec, it very evidently belongs to emphysema, but cannot be its pathognomonic sign; for an analogous sound has been noticed in the commencement of pleurisy and pericarditis, when these phlegmasiæ are yet without effusion. But of what importance is it? the comparative signs of auscultation and percussion, which I have just laid down, perfectly suffice to establish the presence of pulmonary emphysema, and to distinguish it from other affections with which it is liable to be confounded.

Now, let us admit that Laennec has greatly magnified the number of cases in which asthma had no other cause than pulmonary emphysema; who will not pardon him for this exaggeration, so natural to a mind like his, which discovering an anatomical alteration till then unknown, and being enabled to give it so clear and evident a symptomatology, believed itself authorised in thinking that many cases of pulmonary emphysema had passed unperceived, and had been described as cases of essential asthma? Besides, Laennec has not said that all cases of asthma were emphysema; and as a proof of it, he has devoted a chapter of his work to the study of *spasmodic asthma*, the name under which he designates that form which no anatomical lesion can account for, and which he regards as an essential neurosis.

But pulmonary emphysema is not the only affection that can be detected by means of auscultation, and that is capable of producing attacks of asthma.

Laennec has described, under the name of *dry catarrh*, a disease consisting of a chronic inflammation of the bronchiæ, which is accompanied by no expectoration, or at least by very little. This disease is often concomitant with asthma, in cases where it is not its primary cause. This affection, common in old gouty persons, hypochondriacs, and those whose constitution has been injured by excess, presents as its principal symptom a dyspnœa that affords all the characters of that constituting asthma, and is always confounded with it, inasmuch as it returns by attacks which last several days. Towards the end of these attacks, a cough supervenes, and then the oppression diminishes. After several days, the efforts of coughing produce pearly sputa, whose expectoration causes a still more perceptible diminution of the dyspnœa. The stethoscopic sign which Laennec has allotted to it, is: diminution of the respiratory murmur without a bronchial râle, coinciding with natural sonorousness of the chest. When dry catarrh is protracted during a certain time, it ordinarily engenders pulmonary emphysema, an affection which we have seen to be a frequent concomitant of asthma.

M. Quissac, then chief house-surgeon of Hotel-Dieu at Montpelier, first called the attention of practitioners to a disease previously undescribed, in an excellent monograph published in the *Gazette Médicale de Paris* of 1836 (pages 161 and 177). This disease, to which he gives the name of contraction of the lungs, has for its anatomical character the horn-like induration (*racornissement*) of the lungs, whose density increases in direct proportion to the smallness of the volume to which they are reduced. If this examination be pushed farther, we find that the mucous membrane of the bronchiæ exhibits traces of phlogosis, and that the cavity of these conduits is effaced in their ultimate divisions. The symptoms of this affection may be confounded with those of asthma when they are acute, and are then only distinguished by the brief duration of the malady compared with the essentially chronic course of asthma. But when the contraction assumes a chronic course, the rapid and complete wasting which a want of hematosis induces,

would make it rather resemble phthisis; and it is, indeed, among this class of diseases that M. Quissac appears willing to place it, from his calling it *phthisis by contraction.* The stethoscopic signs which he gives to it, are: absence of the respiratory murmur, and great sonorousness of the thorax. These are identical with those of pneumo-thorax, unattended by effusion into the pleura; but it must be confessed that M. Quissac has not sufficiently distinguished these two diseases from each other. It is very evident, in the observations which he cites, that the contraction is essential, and that it does not resemble the simple compression of the lung by an effusion of air or pus. But M. Quissac thinks that the pleura, in this case, contains nothing, not even gas: which contradicts all the received ideas of animal physics. However, this newly described disease requires to be studied anew; and the signal once given, observation aided by pathological anatomy, will do the rest.

Drs. Kopp and Hirsch of Kœnigsburg, were the first to describe a disease which has since been generally called, in Germany, *thymic asthma.* This disease, peculiar to infancy, and the cause of which, as its name denotes, consists in hypertrophy of the thymus gland, is characterised by fits of suffocation during which respiration is almost suspended, and which return periodically, especially at the moment of waking, during deglutition, and when the child cries. The first memoir *ex professo*, on this disease, was read by Dr. Kopp, at the assembly of learned German naturalists at Heidelberg, in September, 1829. From the description given by the authors of the symptoms which characterise this disease, we see at once that it has the name only in common with asthma, and that there would be much more danger of confounding it with hooping-cough. Its physical signs are: five or six inspirations, at first hissing, then deeper and more painful, alternating with a scarcely perceptible respiration, whose sound bears some resemblance to that of croup developed to a high degree. The acute cry heard at the commencement of inspiration is the pathognomonic sign of this affection. Another

German physician, Dr. Roesch of Schwenning (Wurtemberg), who has been much engaged in observing thymic asthma. without denying that hypertrophy of the thymus has been encountered in all cases, does not, however, make it the essential character of this disease; in his view, this essential character consists in a spasm of the glottis. He considers it, therefore, as constituting a true neurosis, and he would be tempted to call it convulsive asthma of the larynx. He founds his opinion on the fact that the symptoms which Drs. Kopp and Hirsch have assigned to it, are not those which would indicate compression of the lung, and a mechanical obstacle to the circulation of the blood and the movements of the heart. There is neither asphyxia, lipothymia, nor cyanosis: the fits. instead of being developed during the motions of the child, return, on the contrary, when he is in a state of repose.

It would be difficult for me to decide so obscure a question; but it no less follows, from all these observations, that there is still a species of asthma of which percussion and auscultation united may enable us to suspect the material cause during life, before dissection reveals it.

I stop here in the enumeration of the thoracic affections regarded as capable of producing attacks of asthma; for the others are acute or chronic diseases, having their peculiar symptoms, completely distinct from asthma; and if the anatomical characters have sometimes been observed in asthmatics, there could have been simple concomitance only, but by no means reciprocal dependence.

I have said that angina pectoris had been regarded by most authors, as specially allied to organic affections of the heart; I have already shown how little precise information was furnished by auscultation, for detecting the true nature and seat of these lesions. I should add, that were we enabled to diagnosticate in the most exact manner an organic affection of the heart or the great vessels co-existing with angina pectoris, we still would not for all that have *localised* the seat and determined the cause of the group of symptoms denoted by this word. This very clearly and evidently results from a work

entitled: *Nouvelles observations sur la nature et la siége de l'angine de poitrine*, published by M. Gintrac of Bordeaux, in the *Journal de Médicine pratique*, or *Recueil des travaux de la Société de Médicine de Bordeaux* (1836). We there read the history of a man, in whom dissection disclosed four aneurismal dilatations of the sub-sternal portion of the aorta. Now, this man had been subject for many years to attacks of angina pectoris, but had been completely relieved of them ten years before his death, and had not afterwards felt them. It is shown by this most interesting case, that other conditions than an alteration of the aorta are necessary for the production of this painful disease. M. Gintrac thinks that the nerves of the cardiac plexus are probably affected, and that they are the cause of the violent pain complained of by patients. This opinion will certainly be shared by the great majority of practitioners.

There is then in asthma and angina pectoris a nervous element which no one can doubt, and which renders them in some degree independent of the organic lesions with which they may coincide. Does it follow that auscultation and percussion have not rendered an immense service by facilitating the diagnosis of these lesions? Who would dare maintain this? It is incontestable that in employing a direct treatment against dry catarrh or emphysema, by means of kermes mineral, ipecacuanha given in doses sufficient to produce vomiting, medicinal soap associated with squills and sometimes with gum ammoniac, salts with a predominance of alkali, and sometimes oily frictions on the chest; against contraction of the lungs, by means of bleeding and exercise commended by M. Quissac, as excellent in the treatment of this disease; against engorgement of the thymus gland, by venesection frequently repeated; and lastly against every disease of the heart recognised by auscultation, were its nature suspected only, by emissions of blood, baths and digitalis, we will succeed in disengaging the nervous affection from a complication which could but aggravate it, by causing the attacks to return more frequently than they otherwise would. It often hap-

pens, indeed, that by combating the intercurrent diseases, we will thereby at the same time relieve the nervous disease, and without there being any necessity of employing the special remedies which experience has taught to be the most efficacious in its treatment.

But if a rigid investigation of the pectoral organs, by means of auscultation combined with percussion, does not enable us to recognise any lesion whatsoever of those organs, this favorable result will nevertheless follow from such an examination; that in treating essential asthma by suitable means, we shall be sure of not leaving untreated an affection which, although secondary in appearance, would nevertheless countervail the action of narcotics or other remedies directed against the nervous affection: for we may be certain, that the diminution of the attacks will take place only after the organic affection, whatever it may be, shall have been relieved; and that this could not be until after the latter had been recognised, is perfectly plain.

CHAPTER VI.—RECAPITULATION OF THE PRECEDING CHAPTERS: ARTIFICIAL AUSCULTATION.

After having considered in succession all the diseases of the lungs and heart, and detailed all that diagnosis, and consequently therapeutics, owe to auscultation, it seems to me natural to bring together in one view, by a summary and brief recapitulation, all the improvements which the science of diagnosis has made since the admirable discovery of Laennec. Such a review can but redound still more to the glory of that powerful mind which has created a new symptomatology for so numerous a class of diseases, and thus rendered their diagnosis as simple and easy as before it was obscure and confused. This will be entering still more into the spirit of the programme of this *concours;* for it seems to me, in proposing it to the zeal and emulation of the young physicians of France, the Bordeaux Medical Society wished that there should result a monument to the glory of Laennec, recounting alike the

importance of his immense labors and the gratitude with which all practitioners of all times should preserve his memory.

From the exposition which I have given, it results that the immediate consequences of auscultation combined with percussion are the following:

1. A distinction for ever established between pleurisy and pneumonia, diseases which Laennec has shown to differ as much in their physical signs as in their anatomical characters.

2. The diagnosis of pleuritic effusions based on signs so exact, that henceforth any error is impossible: an inestimable service rendered to surgeons, who, previously, possessed so few means for detecting cases of empyema, that in most instances they doubted their diagnosis until the issue of pus from the chest.

3. The diagnosis of pleurisy, established at its onset, rendered independent of the general signs of reaction: so that, to the attentive practitioner, there can no longer be latent pleurisy, any more than latent pneumonia, for what we say of one of these diseases is equally applicable to the other.

4. The discovery of lobular pneumonia, which without auscultation would never have been made: for either the patient gets well, and then nothing reveals the origin of the hæmoptysis which was the sole symptom; or death takes place, and then it is only subsequent to the extension of the phlegmasia to the whole lung, which causes the disappearance of that singular state of the organ to which the name of lobular pneumonia has been given.

5. The discovery of hypostatic pneumonia or pneumonia of the dying; a disease which pathological anatomy had already pointed out, but which had been considered a cadaveric phenomenon. Auscultation has enabled us to detect it during life, and has thereby furnished indications to prevent, and sometimes to relieve it.

6. A differential diagnosis irrevocably established between pulmonary catarrh and phthisis, diseases which are confounded, so to speak, by their general and almost by local symptoms

(except, however, those furnished by auscultation), and which the greater number of practitioners until then regarded as two stages of the same affection, differing only more or less.

7. The establishment of a strong proof against the generation of phthisis by chronic pneumonia, by showing that these two diseases, in the generality of cases, commence inversely, the one at the summit, the other at the base of the lung.

8. The diagnosis of pulmonary cavities resulting from the softening of tubercles, established on such precise and sure data, that not only can we determine their number and extent, but likewise indicate in a certain manner whether they are completely empty, whether they contain pus alone, or air and pus both at once, whether they are deeply situated in the parenchyma, or near the surface of the lungs, etc.

9. The demonstration, until then never furnished in so evident a manner, of the possibility of the cicatrisation of pulmonary caverns, by the approximation of their walls or their reduction to the state of fistulæ. Until that time patients who had escaped from diseases of the chest, were believed not to have had phthisis; which is equivalent to saying that the ancients were not sure of their diagnosis until they saw it confirmed by dissection.

10. A certain diagnosis established in the case of pulmonary emphysema, œdema of the lungs, and dilatation of the bronchiæ: lesions which pathological anatomy had been content to ascertain, or rather had suffered to pass almost unperceived, so far were pathologists from thinking to detect them during life.

11. A diagnosis created for pneumo-thorax; an affection which until then had been regarded as a cadaveric lesion only. Signs furnished for ascertaining whether it is complicated with perforation of the lung, or whether there is still no communication of the cavity of the pleura with the bronchiæ.

12. The facility of following step by step, and with the finger and the eye, if I may say so, the daily progress of acute and chronic diseases of the lungs, in their period of increment as well as in their declension: an inappreciable advantage,

which no other method had ever furnished, and which permits us to judge from day to day whether the medication employed should be continued or modified.

13. The discovery of the existence of latent catarrhs, in most cases of continued fever: a fact which had never been, and without auscultation, could not have been noticed.

14. The power of recognising, during life, the organic affections of the lungs and heart which may coexist with nervous diseases, such as asthma and angina pectoris: a knowledge which may lead to the means calculated to relieve, if not to cure those diseases.

In view of such results, who would dare oppose the eulogies which they ought justly to excite, by alleging that auscultation has to this day failed in all the trials which have been made to obtain by it pathognomonic signs of equal certainty for diseases of the heart? True, it has failed, but yet not completely; for auscultation of the heart has discovered sounds in this organ, different from its normal and physiological sounds, and which all are agreed to regard as morbid. Although we may not agree on their intrinsic worth, as signs representing such or such a lesion, they are not the less valuable, inasmuch as their combination with other signs which the patient may present, enables us, by induction, to suspect organic alterations of whose existence it was formerly morally impossible to have any knowledge, other than by dissection. Furthermore, I have stated the source of this uncertainty in the results of auscultation of the heart; I have proved that the want of an unanimously adopted theory for the normal sounds of that organ, was the sole cause of the difficulty experienced in suitably interpreting the normal sounds. Should we then despair of one day seeing this difficulty overcome? And after all, though this consideration may be of moderate value, and I by no means wish it to be inferred from it that we ought to stop here in these inquiries, and not endeavor to solve this last part of the problem of locating the diseases of the thorax by means of auscultation; after all, I say, the organic lesions of the heart are, perhaps, of all diseases, those whose precise location, when detected during life, will furnish the fewest new

therapeutic indications to the practioner entrusted with their management. For, up to this day, the therapeutics of the heart have presented, and perhaps will not present for a long time to come, but these two indications: 1. to diminish the mass of blood by general or local bleedings and a suitable dietetic regimen; 2. to moderate the movements of that organ, by means of digitalis and hypnotics; to which may be added this one: to combat the accidental symptoms in proportion as they may present themselves.

It may be said confidently, and without fear of contradiction, that no one before Laennec had made such immense and unquestionable improvement in the art of diagnosis: no one had settled conflicting opinions on so great a number of diseases, had so irrevocably decided such a number of questions until then regarded as incapable of solution; no one had so completely renewed the history of an entire class of diseases as obscure as those of the chest; and all this Laennec has done through his stethoscope; all this has been the consequence of a single idea, that of listening to the sounds of respiration, and of studying the modifications impressed upon them by the numerous and varied diseases which may alter this function. So true is it that genius does not so much consist in a happy conception, as in knowing how to deduce from it all the consequences which may be drawn.

It was a deep conviction of the indispensable necessity for young physicians to possess a perfect knowledge of this art, which suggested to M. Pétrequin, chief surgeon of the Hotel-Dieu at Lyons, the idea of teaching it to students by the method which he calls *artificial auscultation*. This method, which he has developed in a memoir presented to the Academy of Sciences, the 23d January, 1837, and since published in the *Annales de la Société des Sciences médicales et naturelles de Bruxelles* (1838), consists in first exploring detached lungs, sometimes healthy, sometimes diseased, which he inflates by imitating the respiratory motion, which permits the healthy and morbid sounds to be heard. As to the tubal râles, M. Pétrequin obtains them by throwing various injections into the bronchiæ.

Starting from these data, M. Pétrequin then practices auscultation on the dead body, and assures us that he has succeeded in perceiving the different bronchial and pulmonary sounds, the amphoric respiration, etc.; and thus to have been able to diagnosticate pneumonia, hydro-thorax, cavities from phthisis, and perforation of the lung, upon subjects of whose history he was ignorant. Lastly, to obtain artificial auscultation of the voice, M. Pétrequin applies the widened extremity of the stethoscope on the larynx of a person speaking in a loud tone, and the other end of the cylinder upon the origin of the bronchiæ of the subject ausculted, and the immediate result of this ingenious process is the production of cough and of voice in the brocho-pulmonary cavities.

This theory of artificial auscultation is developed in the memoir which I am analysing, with the spirit and talent remarkable in all the productions of M. Pétrequin, and deserves to be favorably received. It is certainly a happy idea to have mechanically reproduced upon the dead body the circumstances which during life give rise to this or that sound, and I know that these artificial sounds bear the greatest resemblance to those which they are intended to represent. Artificial auscultation may then assist very much in training students to this difficult art, of distinguishing normal or pathological sounds which occur in the chest; but can it completely supply the place of the auscultation of patients? I think not. It is, in fact, on the patient only that the student will learn the connection which exists between the physical signs obtained by the aid of the stethoscope, and the other local and general symptoms, which the dead subject cannot present: such as those drawn from the state of the respiration, the nature of the expectoration, the fever, etc. Let us not deceive ourselves; the art of auscultation does not consist only in hearing the morbid sounds well, but also in properly interpreting them; and this interpretation will be so much the more exact and near the truth, the more it accords with all the other symptoms. Now I admit the inconvenience that sometimes occurs, of submitting patients extremely ill, those affected with pneumonia, for example, to the observation of a large number of students,

who, not knowing as yet how to auscult, fatigue them much more than a more practised physician. But it is the duty of a prudent and wise clinical professor, to reconcile the interest of the patient with the advancement of the student: a thing that is easily done, and obtains every where.

As to the advantage which artificial auscultation might have of enabling the student to learn this art much quicker than by the ordinary method, which, according to M. Pétrequin would require one year of assiduous attention in a hospital to acquire the skill necessary to prevent error, I confess that it does not seem to me of very great importance; for, admitting that a whole year would be required to attain a sufficient degree of skill in the art of auscultation, is this time too long, compared with the importance of such a study? Besides, this year would not be exclusively devoted to this subject, but would in reality be a year of clinical instruction, during which the student would learn to recognise and to treat, not only the diseases whose diagnosis is obtained by auscultation, but also all others. The problem of medical education does not consist in abridging the duration of study, and causing the candidate to pass rapidly through the series of examinations which is to conduct him to the doctorate, but on the contrary, in making him spend most profitably the years which the legislature has wisely ordained as the duration of this species of novitiate, in order that when this time has expired, he shall present himself for the final trial with sufficient instruction to serve as a guaranty to the society which is about to grant him such great powers.

CHAPTER VII.—OF AUSCULTATION APPLIED TO CASES FOREIGN TO DISEASES OF THE CHEST.

It was natural to expect, that, as soon as the beautiful discovery of Laennec became known, a crowd of imitators would follow in his steps and endeavor to discover, by the aid of the stethoscope, every sound capable of being produced in our economy. Laennec himself was led to make researches with this object in view; but all these trials were not equally

successful, and there are some indeed of which I shall say nothing, such as those which concern abscesses of the liver and the tympanum, the diagnosis of which Laennec had thought to render more easy by this method, which experience has shown to be impracticable. I shall glance rapidly at the other applications which have been made by other authors and in various cases.

1. *Auscultation of fractures.*—M. Lisfranc is the first, and I believe the only one who has made experiments on this subject. He published them in a memoir inserted in the *Archives générales de Médecine* for August, 1823. There was nothing very surprising that the application of the stethoscope should cause the crepitation of the fragments to be more easily and distinctly heard, since this noise generally reaches the naked ear, and is numbered among the signs of fractures by all authors on surgery. But what characterises the work of M. Lisfranc, is the determining of the distance from the fractured point at which the stethoscope would enable the crepitation to be heard. Now, he has found that the sound is propagated to points quite distant from the place of its production, and indeed, in certain cases of fracture of the femur, he has heard it by placing the stethoscope on the cranium.

M. Lisfranc pretends to recognise, by the difference of the sounds heard, whether the fracture be oblique or transverse; if there are several spiculæ, whether there be a liquid extravasated around them. Laennec, who sought to verify the experiments of M. Lisfranc, avers that he has distinguished, by the aid of his stethoscope, the pointed or obtuse form of the fragments, when the thickness of the soft parts did not permit it to be recognised in a precise manner by the hand. In reading such results, which no one has since attained, we cannot help thinking that these authors have somewhat exaggerated them, by a natural tendency of the mind to augment the importance of a new idea on which great hopes are founded.

These hopes have not been realised; by common consent, they are here stationary; and, really, there are few cases

where the able surgeon cannot detect a fracture by the ordinary means of investigation; and in difficult cases, where the diagnosis is doubtful, in spite of the most skilful exploration I do not think that the information furnished by the stethoscope would be sufficient to dispel all doubt, and hence its application is useless.

II. *Auscultation of the bladder.*—The object of catheterism practised for the detection of a vesical calculus, is to render sensible to the ear and to the finger conducting the sound, the shock of this instrument against the stone: it is only on this two-fold condition that we can declare the existence of stone in the bladder. But very often this double sensation is doubtful, and the examples, unfortunately still too frequent, of patients cut without having stone, is a sad but convincing proof of it. Laennec has proposed in these ambiguous cases to apply a stethoscope on the pubis, whilst another person explores the bladder with a sound. But still in this case, it is possible that the sound produced by this shock may not reach the stethoscope, and consequently the ear of the observer. It is to obviate this inconvenience, a necessary consequence of the interruption which exists between the place where the sound is produced, and the conductor which should bring this sound to the ear, that M. M. Moreau of St. Ludger and Béhier, have thought to make the ivory plate of the stethoscope, on which the ear is placed, communicate with the flat extremity of the sound by a metallic wire, by means of which the noise is directly transmitted from the extremity of the sound which percusses the stone, to the ear of the observer. These gentlemen have published the essays made by them on this subject, in the *Journal des connaissances médico-chirurgicales*, of 15th April, 1836, and in the *Lancette française* of 7th May, the same year.

M. Leroy d' Etiolles, finding this rigid conductor very inconvenient, inasmuch as the movements which we are obliged to impress on the sound constantly displace it, has modified this instrument in a very ingenious manner, by replacing this conductor by a spiral spring covered with caoutchouc, which

renders it flexible, and consequently susceptible of yielding to all the necessary movements, without injuring the transmission of the sounds, the perception of which it must render more easy. It was on the 31st July, 1837, that M. Leroy d' Etiolles presented his new instrument to the Academy of Sciences. This process, added to the numerous ameliorations which lithotrity has affected in the methods of catheterism, will certainly contribute to render errors of diagnosis more and more infrequent, if it do not altogether prevent them.

III. *Auscultation of aneurisms.*—The auscultation of aneurisms was a natural consequence of that of the heart; but the information which it affords in these diseases has not a very great value, and can by no means elucidate a doubtful diagnosis. Indeed, in cases of aneurism, where any thing is heard, it is a rasping or bellows sound, resulting from the entrance of the blood into the aneurismal sac. But this sound has no scientific value; it proves that the circulation is impeded, and that the wave of blood which presents itself does not find an aperture sufficient to pass beyond; but the compression of an artery by a superincumbent tumor would produce the same effect. Now, the sign which auscultation ought to furnish, would justly be a differential sign, serving to distinguish an aneurismal tumor from a tumor foreign to the artery. I will say almost as much of the peculiar noise designated by the name of *susurrus*, which is sometimes heard in false consecutive aneurisms, and has been attributed to the passage of the blood through the narrow opening which establishes a communication between the cavity of the artery and that of the aneurismal sac. This *susurral* sound likewise shows nothing more than an impediment to the circulation, and the passage of blood through a too narrow space. But in order that it should be really significant, this impediment to the circulation must not depend on any other cause than the aneurism; and we have just seen the contrary.

IV. *Auscultation of the brain.*—The idea of applying the stethoscope upon the cranium, and of thus obtaining pathognomonic signs in diseases of the encephalon, did not occur

to any one before Dr. Fisher called the attention of practitioners to this point, in a note read in 1833 before the Boston Society for Medical Improvement, and inserted in the *Medical Magazine*, No. 15, an analysis of which is given by the *Gazette medicale de Paris* of 14th January, 1834. Dr. Fisher has obtained by this process a phenomenon not yet noticed, and which consists in a bellows sound which has always appeared to him to coincide with compression of the brain. By analysing the six observations which he has annexed to his paper, we see indeed that two of the patients on whom he observed the encephalic bellows sound were affected with acute hydrocephalus, two with chronic hydrocephalus, the fifth labored under the effects of a fall from a second story, and the sixth suffered with an organic disease of the brain. In all it was ascertained that the bellows sound, heard in the region of the encephalon, did not proceed from the heart, since it was not heard in the præcordial region. Three patients having got well, it was found that the bellows sound decreased in proportion as the disease diminished in intensity, and disappeared when the convalescence was complete.

Dr. Fisher, in some reflections which are remarkably just, then makes the following deductions: 1. that this sound takes place in the arteries of the brain; for it is synchronous with the pulsations of the temporals and carotids, and the compression of these vessels causes it to cease; 2. that it occurs in the arteries of the base of the cranium, because it is in that place that the somewhat voluminous arteries are found, and where only they are so situated as to be compressed between the cerebral mass and the bones forming the base of the encephalic cavity; 3. and lastly, that it always accompanies compression of the brain.

As to this last conclusion, more than six observations are necessary to establish it. But the diagnosis of encephalic diseases is so obscure, that the indication of a new sign capable of improving it, is indeed an immense service rendered to science. Time and further inquiry will show what we are to expect from this new application of auscultation.

V. *Auscultation of the abdomen.*—Dr. Bright, physician to

Guy's hospital, at London, first pointed out a *sound like new leather* (*bruit de cuir neuf*), which is perceived by the touch and by the ear, in examining the abdomen of certain patients affected with chronic peritonitis. Dr. Corrigan has confirmed these remarks, in an article inserted in the *Dublin Journal of Medical and Chemical Sciences* (1836). But whilst Dr. Bright attributes it to the rubbing of the adhesions established between the two expansions of the peritoneum, Dr. Corrigan thinks, on the contrary, that it ought to cease as soon as these adhesions are formed, and that the only condition capable of producing it, is the deposite of a certain quantity of lymph, moderately thick and consistent, on the surface of the peritoneum. So soon as this lymph grows thick, in order to be organised into false membranes, this sound diminishes, to disappear completely when the adhesions are perfect.

The observations of these two celebrated English practitioners require confirmation; they would furnish a valuable sign to judge of the progress and state of so grave a disease, the diagnosis of which is not always as easy as one might think.

VI. *Auscultation of the sheaths of the muscles and tendons of the fore-arm.*—M. Lalesque of La Teste (Gironde) has inserted in the *Bulletin médical de Bordeaux*, for 1836, a case of rheumatic inflammation of the muscles of the fore-arm, which was accompanied by a rubbing perceptible to the hand which grasped the affected wrist, and by a creaking sound (*bruit de cuir neuf*) which was heard at a distance, and which the ear, applied to the fore-arm, perceived to be very loud. The observation of M. Lalesque, which is extremely curious, is wanting in some details necessary for knowing where this creaking sound took place. The author appears to think that it was in the cellular sheaths which separate the fleshy fasciculi of the muscles of this region; and that the sound which he heard was only the crepitation perceptible by the ear and by the finger, pointed out by MM. Rognetta, Velpeau and Maingault in inflammation of these tendinous sheaths. (Memoir of M. Rognetta, in *Gazette médicale de Paris*, 1834, p. 596).

VII. *Auscultation during pregnancy.*—It remains for me to speak of the most happy application which has ever been

made of auscultation: I mean its application to the study of pregnancy.

It does not enter into my plan to discuss whether this idea was original with M. Kergaradec, or whether he borrowed it from others: his priority has nevertheless been contested. Thus it has been said that M. Mayor, of Lausanne, had long previously had the idea of applying the ear upon the abdomen of the mother, to ascertain whether the child was living; which he determined by hearing the pulsations of the fœtal heart. which were very easily distinguished from the pulse of the mother. (*Rapport fait à l Institut,* on Laennec's work *Auscultation médiate,* by Percy). But M. Mayor did not extend his researches farther. It has also been said that Fodéré had somewhere spoken of it; but to M. Kergaradec will always belong the honor of having first called the attention of accoucheurs to this important point in obstetrical practice.

The stethoscope, applied on the abdominal parietes of a pregnant woman, permits two sounds to be heard: one, somewhat similar to the ticking of a watch wrapped in many cloths, is the *sound of the fœtal heart;* the other is a true bellows sound, which M. Kagaradec has designated under the name of *simple pulsation with blowing* (*battement simple avec souffle*) or *placental sound,* because he supposed this sound to occur in the placenta or in that part of the womb where it is attached. Let us study these two sounds separately.

All accoucheurs are unanimously agreed that the first sound is really owing to the pulsations of the fœtal heart. These pulsations, always double like those of the adult heart, are only much more rapid, and their frequency is commonly twice that of the pulse of the mother. Professor Paul Dubois, in an excellent memoir on this subject, read before the Academy of Medicine, November, 1831, has found the average frequency of the pulsations of the fœtal heart, observed on three hundred pregnant women at the *Maternité* of Paris, to be one hundred and forty or one hundred and fifty pulsations per minute. M. Nœgelé, the younger, in his work entitled *Auscultation obstetricale,* published in 1838, has found the average to be only one hundred and thirty-five pulsations per minute, and his re-

searches were made on nearly six hundred women. Moreover, MM. Paul Dubois and Nœgelé agree in affirming that these pulsations present no remarkable difference of frequency, at whatever period of pregnancy they may be explored. M. Nœgelé does not like the comparison which has been made with the *tic-tac* of a watch, and he cannot give a better idea of the first sound than by comparing it to that of the heart of an infant just born. Accordingly he always makes his students commence by the auscultation of new-born infants, in order to accustom them to recognise the fœtal pulsations when auscultating the mother: and, from the perfect resemblance of these last sounds to those of the heart of a new-born child, M. Nœgelé draws one of his strongest arguments to prove that they are really those of the fœtal heart, and that they can not be attributed to any other cause.

All authors likewise agree in saying that before the fifth month it is not possible to hear the pulsations of the fœtal heart. M. Nœgelé, however, pretends to have discovered, by means of the stethoscope, sounds produced by the motion of the fœtus in the amniotic liquid, several weeks before the woman herself felt the first movements of her child.

The place where the double pulsation is heard, necessarily varies with the position of the child: however, it is not necessary that its dorsal region should be in direct relation with the point of the abdominal parietes on which the stethoscope reposes. Had this opinion, which was that of M. Kergaradec, been true, it would have enabled us by an easy induction, to determine the relations of the pelvis of the mother, without the necessity of touching per vaginam. But it follows from the researches of M. Paul Dubois, that it is sufficient for one of the points of the thorax of the fœtus to be in relation with the abdominal wall; it does not even appear necessary that the stethoscope should be placed immediately over the point of contact, for this author has remarked that the double pulsation is always heard in a radius of some inches around the point where it presents the greatest force. This is more directly shown by an experiment made by Dr. Hoefft, and published in an article of a German journal, under the title of:

Remarques sur l'auscultation des femmes encientes, faites pendant les années 1835 *et* 1836 *dans l'Institut impérial de la Maternité à St. Petersbourg;* which I find analysed in the *Gazette Médicale de Paris,* 1838 (p. 808). M. Hoefft enclosed in a small jar hermetically sealed, a watch surrounded by cotton; he then placed the small jar in another vessel full of water which he surrounded with a cloth. By varying the position of the jar in the larger vessel, he discovered that the power of hearing the tic-tac of the watch was modified by the greater or less conductibility of the body transmitting the sound: for, when the watch was near one of the sides of the vessel, it was heard equally well, by applying the stethoscope to one or the other of the extremities of the latter; whilst, when the watch was near the centre of the fluid, it was heard much less distinctly.

If all accoucheurs have been unanimous in admitting the cause of this double sound in the pulsations of the fœtal heart, they have been far from agreeing upon the interpretation which should be given to the second, noticed by M. Kergaradec under the name of the *placental sound,* because he regards it as indicating the situation of the placenta. This is also Laennec's opinion, who, however, does not think it produced by the placenta itself, but by the arterial branch serving principally for the nutrition of the placenta. Laennec founds his opinion on the experiment of Dr. Ollivry of Quimper, who has four times assured himself, by introducing the hand into the womb immediately after the exit of the child, that the point where he had heard the pulsations with a blowing sound before accouchment, corresponded exactly with the attachment of the placenta; and M. Ollivry adds that, if a new proof were necessary for the support of this opinion, it would be found in this fact: that *the blowing sound ceases at the very instant when the umbilical cord is cut.* (Laennec, op. cit., vol. ii, p. 464).

But further researches by MM. Paul Dubois and Nœgelé have shown that the opinion of Laennec and M. Kergaradec is erroneous, and that what these authors have designated under the name of *placental sound,* takes place in the vessels of the

uterus, and ought to be called *uterine blowing* (Paul Dubois), *uterine sound* (Nœgelé). Indeed this uterine sound is subject to frequent change of place, and to be heard sometimes in one spot and sometimes in another, or else in two places at once: the placenta, therefore, is not concerned in its production. This uterine sound is nevertheless of great importance, because, being heard long before the pulsations of the fœtal heart, and as soon as the womb is sufficiently developed to reach the level of the pubis, it may serve to make us suspect the existence of a product of conception in the uterus, if, however, it be not shown that the development of the uterus by any tumor whatsoever, may not occasion the production of a similar blowing sound.

Be this as it may, it nevertheless follows from what has just been said, that auscultation is a valuable means of recognising the life of the fœtus in its mother's womb; the existence of the double pulsation is an incontestable proof of it. M. Nœgelé even lays down the inverse proposition as a principle, and thinks that the non-perception of the double pulsation is a sign of the death of the fœtus. But obstetrical auscultation is yet surrounded with too many difficulties, for one to be always sure, that the sound which is not perceived does not exist; it may be that it does not reach the ear on account of something that eludes observation—this is the only conclusion that can be drawn from its absence.

It was hoped that we might be able, by means of auscultation, to detect multiple pregnancy, and to suspect this kind of pregnancy every time that the double fœtal pulsation was heard at two opposite points of the abdomen. It appears from the Clinical Review of the Obstetrical Institute of Paris for the year 1830–31, by Professor Lovati, that in a woman who presented this double pulsation at the fundus of the uterus on the left, and in the right iliac region at the same time, a double pregnancy was diagnosticated on this single sign, and at the delivery they were astonished to see with what precision the stethoscope had indicated not only the number of fœtuses, but also the position of each of them: for one presented the thighs

in the *fourth position* (left sacro-posterior); the other, the occiput in the *second position* (right occipito-anterior). But, in the end, these double pulsations in divers points of the uterus were heard in so many women whose pregnancy was simple, that the uncertainty of this sign had to be admitted. However, M. Paul Dubois thinks that during labor, and when the rupture of the membranes has permitted the waters to escape, and consequently leaves no chance for the fœtus to change its position, the presence of two children may be discovered; but then the two double pulsations must be heard simultaneously, situated in two opposite points, and consequently the one stronger than the other. M. Paul Dubois, who has sometimes heard them in this case, has always found them synchronous with each other, and thinks that they can be thus simultaneonsly heard only after the rupture of the two amniotic sacs, or at least of one of them.

The discovery of M. Kergaradec has been fruitful in important results. We feel of what interest it is to ascertain the life of the fœtus, before deciding on this or that obstetrical operation. Thus, to give an example, let us suppose a case of procidentia of the cord: when it shall have been replaced in the uterus, how are we to ascertain the success of this maneuvre? the integrity of the double pulsations of the fœtal heart will be the only certain sign, and if it be ascertained, the natural termination of the labor may be awaited, whilst the progressive feebleness of these pulsations would prove the contrary and justify the intervention of art.

I ought, however, in conclusion, to recall the wise precept of M. Paul Dubois, who does not wish that the accoucheur should seek solely in the state of the fœtal circulation reasons for acting or for waiting, because being independent of the brain, it could not often indicate to us the nearly fatal effects already produced on this organ by the protraction, the difficulties, or the accidents of labor. Furthermore, as M. Nœgelé says, "auscultation is not intended to take the place of exploration by touching; it is a valuable auxiliary added to the other means of investigation, and in conjunction with them must furnish to accoucheurs the most certain rules of conduct."

Selections from American and Foreign Journals.

Minute Anatomy of Fatty Degeneration of the Liver.—The author observes, that in order to make the subsequent description intelligible, he will premise a few words on the minute structure of the lobules of the liver.

Mr. Kiernan has well described the *vascular element* of these minute representatives of the organ. It consists of a capillary plexus intervening between the portal and hepatic veins. The diameter of the capillaries in this plexus is very large, being nearly twice that of a blood globule; while the diameter of the capillaries in most other textures is the same as that of the blood-globule, and in some (as the muscular) even less, so that the blood-globules only pass along by undergoing elongation. This large size of the capillaries of the liver, probably, has reference to the deficiency of propelling power in the portal circulation. This *portal hepatic plexus* may be termed *solid*, as it is extended in all directions, and presents areolæ of nearly the same dimensions in whatever plane it is cut. These areolæ are in general not larger than the diameter of the vessels which form them, so that a well-injected specimen might appear to be composed of little else than vessels.

In the interstices of this capillary plexus lies the *secreting portion of the bile-ducts.* If a thin section of an uninjected lobule be examined with a sufficient magnifying power, it is seen to be almost entirely made up of small, irregular, angular particles, each containing a circular or oval nucleus, within which is a minute point or two, the nucleolus. These particles have a determinate outline, are of some thickness, and possess a fine granular aspect. They also contain (which is very remarkable) one, two, or more globules of fatty matter, irregularly placed, and of somewhat variable bulk.

The microscope at once reveals the seat of the *fatty deposite in the diseased state of the organ.* Instead of containing a few minute scattered globules, *the nucleated particles are gorged with large masses of it,* which greatly augment their bulk, and more or less obscure their nuclei.

This simple description developes the whole anatomical condition of the disease, as well as explains its rougher characters, the bulk, the color, and the freedom of the circulation. The particles, lying in the interstices of the capillary plexus, enlarge slowly and equably, in such a manner as to exert no injurious pressure on the vessels, while their new contents impart that peculiar hue which characterizes the disease. It also throws no little light on the nature and source of the disease. It seems to show that the fat is an *increase of a normal constituent,* and not a formation altogether unnatural in kind; thus distinguishing it from the fatty degenerations of other tissues, where fat is deposited in situations from which it is naturally absent. It likewise indicates an increased activity in the secreting action of the liver, for a considerable period before death, though why the *accumulation* of fat should occur within the nucleated particles does not so clearly appear. To explain that fully, will require a more complete knowledge than we yet possess of the chemical affinities at play within these small laboratories of nature.—Mr. Bowman, *Lancet, Jan.* 1842.

Fatty liver—(Extract from the Reports of the Boston Society for Medical Improvement, March 24, 1842.) Dr. Strong exhibited a specimen of exceedingly fatty liver, taken from a child seven months old. For a short time it was nursed by its mother; but, owing to some irregularity in its bowels, which was supposed to be occasioned by the bad state of health of the mother, who was suffering at the time from sore mouth and other difficulties, it was transferred to another nurse. This proving of no benefit, it was weaned. The bowels, notwithstanding, continued in a bad state. The appetite, however, was voracious. It appeared well nourished, bright, and was precocious in its intelligence. A month since, the child began to be sick at the stomach. The discharges became very frequent, eighteen or twenty a day, and, for the most part, of bloody mucus. The abdomen became full and firm, and the child presented all the symptoms of cholera infantum. Treatment, of various kinds, had not the slightest influence upon the dis-

ease. During the last fortnight, the liver was observed to increase greatly in size. The child died of pneumonia. At the autopsy, there was found hepatization of a portion of one lung. There were no tubercles. The mesenteric glands were slightly enlarged. The liver was fatty and greatly enlarged, occupying nearly one half the abdomen.

Two months ago, the only other child of the family died, two years of age. It had never been a healthy child. It had always been troubled with an irritable stomach, and subject to irregularity of the bowels. During the winter it had scarlet fever, from which it was convalescing, had begun to take food, and the attendance of the physician was remitted. Suddenly it was attacked with diarrhœa, and the same symptoms as the other child, under which it sunk and died. In this case, also, the liver was found greatly enlarged and fatty. The mother is a feeble woman, has always been more or less deranged in her bowels, and evidently has now some chronic abdominal trouble.—*N. E. Quart. Journal.*

Researches into the Physical Causes of Metallic Tinkling, or Amphoric Rhonchus. By. M. de Castelnau, House Surgeon (interne) of the Hospitals.—The author commences this paper by reviewing the different theories which have been suggested to explain this phenomenon, and pronounces them all more or less unsatisfactory. The hypothesis most usually adopted, and which attributes the sound to the bursting of an air-bubble on the surface of the fluid effused into the pleural cavity, is regarded by him as equally defective with the others. Experiments which he details have led him to the conclusion, that the formation of bubbles of air at the surface of an effusion is almost impossible, even in those cases where the perforation of the pleura is below the level of the fluid, while its occurrence is altogether out of the question when there exist perforations of the pleura above the level of the effusion.

The occurrence of pulmonary fistula, however, above the level of an effusion is by no means unusual; it is even stated by M. Raciborski to be the case in by far the greater number of instances. Laennec, too, had observed that, after the operation for empyema, in which the puncture is made above the level of the fluid, metallic tinkling is frequently heard; while if the wound had been made too large, the respiration acquires an amphoric sound. This phenomenon can be ex-

plained only by supposing metallic tinkling to be a variety of the amphoric sound; and M. C's experiments on the dead subject have convinced him that such is really the case. He further deduces from them the following conclusions:

1. That the physical conditions essential to the production of metallic tinkling are: *a*, The existence of a tolerably large cavity, containing air, either with or without fluid; *b*, The communication of the external air with this cavity; *c*, The production of sonorous vibrations in the channels by which this communication is established.

2. The causes which gave rise to these vibrations are identical with those which produce moist sounds in general.

3. Metallic tinkling may be called an amphoric rhonchus, with as much propriety as the term amphoric may be applied to the respiration, voice, or cough.

4. Those cases, if indeed any such exist, in which metallic tinkling occurs independent of the above-mentioned conditions, are exceptions to the rule, as are also the theories advanced in explanation of them.

In confirmation of these views, a case is related in which metallic tinkling was heard, and the respiration, voice and cough had an amphoric sound in a phthisical patient. After death the left lung was found to be occupied by two very large cavities, which destroyed the greater part of its substance. A septum, only three or four lines thick, separated the two cavities from each other. The superior was empty; the other contained about four ounces of broken down tuberculous matter, in a semi-fluid rather than a liquid state, and the openings of the bronchi into the cavity were all, with the exception of two, situated above the level of the softened tubercle. Similar phenomenon were observed in the case of a man in whom fracture of the ribs and clavicle was followed by subcutaneous emphysema and pneumothorax. The metallic tinkling and amphoric respiration disappeared gradually as the man advanced towards convalescence; and there was no reason, at any period of his illness, to suppose the existence of fluid in cavity of the pleura. He likewise alludes to a third case, in which a wound in the chest with a knife, though unaccompanied with subcutaneous emphysema, or even of hæmoptysis, was followed by metallic tinkling and amphoric sounds.

In a second paper on the same subject, two other cases are adduced in confirmation of the author's views.—*Archives Générales de Médecine. Oct. et Nov.* 1841.

Diagnosis of Gonorrhœa, in accusations of Rape.—The following is quoted from a recent work on the venereal disease, by Mr. Acton, late externe at the Female Venereal Hospital, Paris, of which M. Ricord is the chief medical attendant. "Every tyro in medicine will at once distinguish what he calls a clap, by means of the symptoms above described, but such a person may not be aware, that a surgeon cannot always decide at once whether a man is suffering under a gonorrhœa or not, provided no discharge be observed, and the lips of the urethra be not inflamed and no stains seen on the linen. M. Ricord gives the following instance of the occasional difficulty. He was ordered by a magistrate to give an opinion, whether or not a prisoner, said to have violated a girl, was laboring under gonorrhœa or not. The accused presented no swelling of the lips of the meatus, on pressure, no discharge came from the urethra, and there existed no traces of any secretion on the shirt. When interrogated, he said that he had made water six hours previously to his examination. As M. Ricord had some suspicion, he ordered him to pass his urine at once, and desired one of the goalers to watch his prisoner; in six hours after, M. Ricord returned and then found undoubted marks of an existing gonorrhœa; the prisoner confessed that he had made water previously to the first examination, and had taken care to remove the secretion as soon as formed by a piece of lint which he had concealed for that purpose."

The Reviewer justly doubts whether gonorrhœa can be present without an obvious vascular fulness of the mucous membrane. This should be examined with a lens. On everting the lips of the urethra, it is either seen florid, with punctuated redness, and a semi-abraded appearance, as if the epithelium were partially removed, or the veins of the mucous membrane are enlarged and tortuous.—*Am. Journ. Med. Sci.* from *Medico Chirurgical Review*, July, 1841,

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Treatment of Old Fractures by Division of Tendons.—Prof. Diffenbach has several times, in old cases of fracture of the patella or the olecranon, where the portions were dragged far apart, divided the adjacent tendons so as to be able to bring the portions together, and, by friction of them one upon the other, to excite such action as might end in the formation of a shorter and firmer bond of union. In some cases, considerable benefit was obtained after all other means had failed; in

others, the result was negative. Two examples are detailed; in one, an old ununited fracture of the ulna, he divided the tendon of the triceps, fixed the upper portion of the bone in its right place by a bandage, and every fourteen days rubbed it well against the lower one: in three months the union was firm. In another example, an old, distantly united fracture of the patella, he divided the ligamentum patellæ and the rectus femoris about three inches above the patella: then, by an appropriate bandage, and constantly drawing the separated portions more closely together, he obtained, at the end of some months, a complete hardening of the interposed substance, and a considerable amelioration of the patient's state.—*British and For. Med. Rev.*, from *Casper's Wochenschrift*, Oct. 2, 1841.

On the treatment of Paraphymosis. By J. Togood, Esq., of Bridgewater.—The incision of the stricture in paraphymosis, is pretty generally practised by surgeons, except when this disease occurs in children, and in such the reduction is often effected by pressure. I have seen repeated incisions fail of relieving the stricture, and leave foul and intractable sores, which have been extremely tedious and difficult to heal. I have always succeeded in reducing a paraphymosis, either in an adult or child, however long standing, without having recourse to the knife, by the following method:—I place the patient against a wall, and take care that he is steadily supported by an assistant on each side; a piece of linen cloth is then laid over the glans, and with my thumbs I knead the blood out of it, drawing the prepuce forwards at the same time with two fore fingers of each hand. A steady perseverance in this plan never fails, and, although the operation is a painful one, the patient is amply rewarded by the rapidity of the cure, which requires nothing more than the application of a saturnine lotion for two or three days.—*Prov. Med. and Surg. Jan.* 15, 1842.

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STATE OF AMERICAN MEDICINE BEFORE THE REVOLUTION.

American medicine has few abler advocates or ornaments than the Drs. Beck, to one of whom we are indebted for an excellent address delivered before the Medical Society of New York on the progress of Medicine before the revolution. They have contributed a full share to carry a knowledge of their profession beyond the sea, and to make it to be respected in all countries where it is cultivated as an enlightened and liberal calling. Dr. John B. Beck, in the address before us, has vindicated the claims of American physicians to some improvements in practical medicine heretofore generally conceded to Englishmen. We give one example from among a number:

"Although the physicians in the colonies generally followed the prevalent practice of the mother country, yet they are entitled to the credit of originating some modes of practice of great value. The most important of these is the application of mercury in the treatment of inflammatory complaints. This practice took its origin as far back as the year 1736, and the credit of originality is generally conceded to Dr. Douglass, a physician of Boston, by whom it was used in the angina maligna which prevailed extensively over the colonies at that period, and committed the most dreadful ravages. By Dr. James Ogden, a respectable physician of Long Island, this practice was extensively applied in the same disease about the year 1749.

The preparation of mercury which was used was calomel. In consequence of the success which attended the use of this remedy in this disease, it was shortly after resorted to in other inflammatory complaints; and about the middle of the last century it was in common use, in this country, in pleurisy, pneumonia, rheumatism, and others of the phlegmasiæ. I am aware that the credit of this practice is claimed elsewhere; but there can be no doubt that in its origin it is exclusively American, and that to our colonial physicians the world is indebted for one of the greatest improvements ever made in practical medicine."

The credit of this practice is generally awarded to Dr. Robert Hamilton of Lynn Regis, but Dr. Beck shows that, from Dr. Hamilton's own account, his attention was not called to the practice until the year 1764, and that it had been in very general use in this country many years before.

The only fault of this address is, that it is too short, and this defect the author, we hope, will remedy by future contributions to the history of American Medicine. It is a subject which he seems to be peculiarly qualified by his studies to render interesting and instructive to his young countrymen. He closes his address in the following exulting language which every American will heartily adopt.

"The revolution accomplished, and an independent government established, a new career was commenced. In common with every thing else, medicine felt the sacred impulse, and during the brief period of our independence, how has the scene changed! Instead of the feeble beginnings of one or two institutions, twenty-three well established medical colleges are now to be found in different parts of our country; every city has its hospitals; a thriving professional literature has sprung up among us, and we can now boast of authors whom we are not ashamed to mention along with those of European birth. What nation ever accomplished so much in an equal space of time, and under equal circumstances!"

Y.

MEDICAL SOCIETY OF TENNESSEE.

The regular annual meeting of this society was held at Nashville on the first Monday in May, and was attended by about its average number of members. Dr. Buchanan, the Vice President, delivered the annual address, on the pathology of fever, which will be published soon among the proceedings of the Society. In the reading, it struck us as one of the most elaborate and logical papers that we have heard

for a good while, and we are a good deal mistaken if it does not enhance the reputation of the author as a sound thinker and a clear and learned writer. Dr. Richardson, of Rutherford, read a practical and judicious paper on the medical topography and diseases of that county, which has been promised us for publication in the Journal. Several of the cases reported possessed practical interest, some of which we expect to publish. Upon the whole, the meeting was characterised by as much spirit as has usually marked the meetings of the Society, and as the attendance has been reduced chiefly to the members who take part in the literary exercises, and believe that the beneficial influence of the association is to be derived from that source, it is probable that it will not decline below its present state, which, if not one of youthful vigor and hope, is yet one of respectable solidity and animation.

The officers elected were, Dr. Buchanan, of Columbia, President; Dr. Geo. Thompson, of Jefferson, Vice President; Dr. R. Martin, of Nashville, Recording Secretary; Dr. Shelby, Treasurer, and Dr. Richardson, of Rutherford, Orator. The transactions of the Society are to be published shortly, when we shall be able to speak more at length of its proceedings. Y.

DEATH OF DR. SAMUEL HOGG.

Died, on the 28th of May, at his residence in Rutherford County, Tennessee, Dr. Samuel Hogg, one of the oldest and most eminent physicians of that State. The disease which terminated the life of this estimable man was pulmonary consumption, of which he had been the subject for a number of years. At one time in the summer of 1838 it was feared he would sink under the disease, but by the autumn he had so far recovered as to be able to return to Natchez, where he then resided, and resume the practice of physic. With the return of warm weather, however, his malady grew more alarming, and he was obliged to fly again to the mineral springs of Tennessee, where the recuperative process went on so kindly, that he was found the next winter, in Nashville, laboring in his profession, and the next spring taking an active part in the business of the Medical Society of the State. At the meeting of the Society, in May of the following year, he delivered the annual address, as President, which was the last public effort of his life. The summer was passed, as usual, at the

watering places in the mountains of Tennessee, and in the winter, his health having sensibly declined, he exchanged his house in Nashville for a farm in the country, to which he retired in March, to sink soon into the grave.

Dr. Hogg was a native of North Carolina, and was born on the 18th of April, 1783, of respectable parents, his father having served his country as Major in the war which achieved our independence, and his mother, who was a widow at the time of her marriage with Major Hogg, having been equally remarkable for her good sense, spirit, and devotion to liberty. It is asserted, on authority believed to be indisputable, that it was this lady, then the widow Webb, who made the reply to Tarlton, memorable for its withering sarcasm, that "he *might* have enjoyed the pleasure of seeing Col. Washington, of which he then seemed so desirous, *if he had only taken time to look behind him at the battle of the Cow Pens.*" Dr. Hogg inherited a large share of his mother's courage and mental activity, and as he lost his father very young he was probably indebted chiefly to her for the early training of his mind. He received instruction, for some time, at a high school in Caswell County, where he acquired some knowledge of the Latin tongue, and became sufficiently versed in the various branches of English education to take charge of a school for boys, which he taught for a short time. Having chosen medicine for a profession, he engaged in the study of it under the direction of Dr. John Hare, and with such aids as his preceptor could afford him, entered upon the practice after the course of reading customary at that day. His first settlement was at Gallatin, in Tennessee, but he was induced, after a few months, to remove to Lebanon, in a neighboring county, where there was less competition. At this place the late war of this country with England found him, and obeying the impulses of his nature, as well as the dictates of duty, he accepted the appointment of Surgeon to the troops that descended the Mississippi to Natchez in 1813.

He returned from this service with an experience greatly enlarged, and with a high reputation for professional boldness, discrimination, and skill. The winter following, in a similar capacity, he accompanied the Tennessee militia on the campaign against the Creek Indians. We have before us an interesting memorial of this campaign, in a brief notice written by him of the measles which broke out among the soldiers in January and prevailed for many weeks as an afflicting epidemic. This account is short, but describes with clearness the

peculiar features of the epidemic, and contains indications of that decision and energy in practice for which Dr. Hogg was especially distinguished. In the winter of 1814 he attended the troops to New Orleans, and was present at the memorable battle of the 8th of January. He was about rising in the morning when the first cannon announced the commencement of the conflict, and it is an interesting incident in his life, that the servant who was handing him water to wash was killed by a cannon-ball which carried away his head, spattering his brains in the Doctor's face.

During the encampment at New Orleans, influenza, in its most violent form, broke out among the troops, and it was in his ceaseless attendance on his afflicted countrymen, bringing an attack of the epidemic on himself, that, according to his own belief, he laid the foundation of that disease to which at length he fell a victim. This complaint appearing in the extreme north-east of the United States, and spreading over the country to the south-west, and termed, variously, cold plague, influenza, pneumonia typhoides, &c., is well remembered by the profession as, perhaps, the most fatal epidemic that has visited the country since its first settlement. Whole families, consisting, in some instances, of fifteen or twenty members, were cut off, and localities the most healthful were visited by it with a mortality not exceeded in cities subject to the visitations of yellow fever or the plague. The plans of treatment pursued in it were as diverse as the localities where it appeared were distant from each other—in some, the practice being to support the sick by cordial remedies; while in others nothing was found to succeed but the most active antiphlogistic treatment. Among the advocates for the latter practice Dr. Hogg was one of the most enthusiastic and successful. One of the sayings which we remember as reported of him during the sway of the epidemic was, "that *his patients should not die of the disease—he would bleed them to death first!*" By universal consent, no practitioner in the army, or in his neighborhood, where he encountered the disease the winter subsequent to his return home, surpassed him in the number of his cures. In truth, his fame as a practitioner rose, in consequence of his success in this epidemic, to a height which has not been exceeded by that of any physician in that State.

To his professional accomplishments Dr. Hogg had the good fortune to add all those qualities of the man which most captivate and win men's hearts. He was brave, confiding, social, liberal to a fault, of conversational abilities the readiest and most agreeable, and with in-

cidents in his own eventful life upon which to exert them to the highest advantage. Soon after his services in the army he was induced by the wishes of his countrymen to become a candidate for civic honors, and once he was chosen the Representative of his county in the Legislature of the State, and once was elected to Congress. It was while serving his country at Washington, in 1819, that the University of Maryland conferred on him the honorary degree of *Doctor of Medicine*, which was followed, some years afterwards, by a similar acknowledgment of his high professional character from Transylvania University.

Dr. Hogg's reputation was now in its zenith, and as he was of an active, inquisitive mind, that acquired with uncommon readiness and ease whatever circulated in any of the channels of the profession, he had only to direct his affairs with prudence, to render his circumstances in life as independent as he could have desired. At this time his services were in perpetual requisition. His connexion with the army during so many campaigns had extended a knowledge of his skill all over the State, and he was consulted by the sick from every portion of it. He almost lived on horse-back, and performed for several years an amount of professional service which has seldom been surpassed by the practitioner of medicine. But labors so arduous and unmitigated began to produce their natural effects upon his health, and in order to lighten them he removed to Nashville, in 1828, where he secured at once an amount of business proportionate to his high reputation. But, having been unfortunate in his connexion with a drug store, he was tempted by the representations of friends to try his fortunes in a field promising still higher pecuniary rewards, and accordingly, in 1836, he settled in Natchez. Here, as before in Nashville, his business became at once large and profitable, and, could his health have been prolonged, it is probable he would have amassed riches. But unfortunately that failed him after a few months, and he was compelled to give up his cherished schemes, only to return to them for a little while once more, and then abandon them forever. In 1838, an invalid ourself, we passed him at Tyree's Springs, where it was apprehended he was about to finish his course. It was during his illness that season that he professed religion, and was received into the Baptist church, in the faith of which he died. We had the satisfaction of seeing him promoted to the first office in the Medical Society of the State which he had honored by his services, at its meeting in May, 1840, and in 1841 listened with mournful interest to his address as the retiring President,

assured by his emaciated frame and feeble voice, that we should hear him from that place no more. When next we saw him he was on his death-bed. A year had passed away, and in that time he had lost a third son, an amiable young man, the second who had prepared himself for the profession of the father. His earthly hopes had all faded, and he spoke of his dissolution as just at hand; but he spoke of it with the calm resignation of a Christian. In three weeks the end, which had been threatened long, came, and he died without a struggle. He was buried at Nashville, a large concourse of citizens attending his remains to the grave. Resolutions respectful to his memory were passed by the physicians of Murfreesborough and Rutherford County, by one of which a memoir of his life and character is requested to be read at the next annual meeting of the State Medical Society, of which he was one of the founders and most active and zealous members.

One of the medical pioneers of Tennessee, Dr. Hogg was too ambitious and too proud a man to lag behind those whose superior opportunities had enabled them to store their minds with the last improvements in the profession. He was, therefore, a reading man. He kept up with the discoveries in medicine, and with the changes in medical doctrine. He loved medical speculation, and spake and wrote fluently concerning his favorite theories, which, if never profound, were generally plausible and ingenious. He possessed a large share of energy and enthusiasm, which he carried into his profession, and these qualities gave him confidence and boldness as a practitioner, and, added to his powers of conversation, rendered him a fascinating teacher. If the character of his intellect was rather showy than solid, it will be conceded that few men, with as limited advantages of early professional learning, have maintained a more undisputed empire in their profession, than Dr. Hogg long exercised in his comparatively limited sphere. As a writer, he was not wholly unknown to the profession; but he was too sensible of the defects of his early education, to be a willing contributor to the press. As a member of society he was greatly beloved, his warm and benevolent heart attaching men to him by the strongest ties. It was late in life that he united to the generous qualities of the man the high and ennobling graces of the Christian; but it is the richest of all the sources of consolation left to his friends—and above all, to his bereaved family—that he was supported under the sufferings of his last lingering illness by the Christian's hopes.

Y.

NOTICES OF THE WEATHER AND DISEASES IN THE WINTER OF 1841–2, AT PARIS, STARK COUNTY, OHIO, IN A LETTER TO THE SENIOR EDITOR FROM C. H. PRESTON, M. D.

"In order to fill my sheet I will give you a synopsis of the character of our past winter and its attendant diseases. As to the former, suffice it to say that it was somewhat remarkable for variableness—much rain, litttle snow, no sleighing, and the lake not frozen over. The prevailing winds were from the east and north east, and have continued during the spring months, and up to the present moment. Vegetation commenced nearly two weeks earlier than usual, but for the last four or five weeks the weather has been (frequently at least) disagreeably cool. Agricultural prospects are, however, quite promising.

Ours is ordinarily a very healthy region, there being few sources of miasm, and the face of the country generally somewhat undulating, with an abundance of pure running water—the spring and wells somewhat impregnated with lime. Our diseases, then, are generally such as are engendered by sudden vicissitudes of weather, which are frequent and sudden, owing in part to our position, in common with most of the northern part of the state, in a region where the influences of the northern lakes, the Allegheny mountains and the Mississippi valley react. These diseases consist of pneumonias, and other inflammatory affections, with now and then an epidemic of typhoid pneumonia, scarlatina, dysentery, &c. Bilious fevers are not prevalent to a great extent, or of a severe grade among us, excepting on the Ohio canal. But to the epidemic of the last winter. The season was as healthy as usual till the month of December, when more than the usual number of cases of pneumonitis and acute hepatitis occurred, with other inflammatory affections, the result of exposure or the sudden changes which began to visit us thick and fast. About the first of January the scarlet fever made its appearance both in this and in several of the adjoining counties at the same time. Its appearance and spread were evidently independent of contagion, no precaution or remoteness affording any security. As far as my observations extended it was generally of the congestive or congesto-inflammatory grade, and proved peculiarly unmanageable. A number of cases, evidently the result of the same great epidemic constitution or influence, occurred, in which the little patients were taken off in from 16 to 24 hours after the first appearance of indisposition. In these cases the nervous system, and particularly the brain, sustained the burden of the shock.

A pale or purplish appearance of the surface, shrunken features, frequent, feeble or depressed pulse, frequent vomiting of a greenish or purplish fluid, absence of expression in the eyes, the conjunctiva injected, cool feet and preternaturally warm head, clammy, slightly furred tongue, and before death, convulsions; these cases thus rapidly fatal—the patient being sometimes dead, sometimes in articulo mortis when I was called in—could not be termed scarlatina proper, as there was no eruption or sore throat, the essential characteristics of that disease; but from the identity of the general symptoms I have no doubt of the identity of their general nature, one great constitutional predisposition acting as the moulding and controlling agent over them all.

I need not trouble you with the treatment, for in these cases it was of little avail. Warm baths, small portions of cal. and ipecac., stimulating injections, warm drinks (in genuine congestion), cold to the head, sinapisms to the feet, blisters between the shoulders, stimulating cataplasms to the abdomen—in short, such means as I deemed proper to divert the circulation from the brain, internal large vessels and other vital organs, and to equalise the circulation. Cupping the temples under similar circumstances in scarlatina was sometimes found very beneficial in relieving the head.

As to the scarlatina proper, I observed that it was mostly of the congestive or congesto-inflammatory grade. The treatment, of course, would not differ much in its general principles from what I have detailed above. In fact, very little medication was found necessary or of much avail—good nursing being of equal importance.

The disease in this vicinity was not as much inclined to result in severe ulceration of the throat, ears, &c., as I have frequently observed it to be, though several such cases occurred. The shock was sustained by the lungs, brain, and general system. The proportion of deaths generally was somewhat greater than in common epidemics of this disease. It continued with little abatement through the months of January and February, and was confined principally to children. During the latter part of the epidemic the measles accompanied it, and became quite prevalent. They were observed to be more obstinate than usual, owing to the general depression of the vital organs. It was difficult, in many instances, to obviate their tendency to settle on the lungs in the form of congestion or inflammation; and although there was no death from them in my practice, they proved extensively fatal in some neighborhoods not far distant, many adults as well as

children being carried off by them. Blood-letting proved beneficial in most cases of this disease where an inflammatory tendency existed, a modification that I thought did not generally obtain, very few patiently as a general rule bearing the remedy to much extent.

At this time, and for some weeks past, the sunshine of health is dispensing its welcome influence over our country; and the afflictions of the past are giving way to the interests and engagements of the present."

June 8th, 1842.

THE WESTERN LANCET.

The editor of the Western Lancet, published at Cincinnati, evinces much surprise at an editorial article, entitled "Western Periodicals," in our June number, and seizes the first moment of returning tranquillity to read what he considers, no doubt, a short lecture, to the press generally and to our senior editor and this Journal in particular. We regret not having space for the whole of his remarks, as we would like the reader to see what a raw-head-and-bloody-bones a man's distempered fancy can create out of nothing.

In speaking of the periodicals published in the West, we made the following observations: "These journals are in the interests of the three medical schools in the West; and may be expected, respectively, to advance those interests by every means consistent with *truth* and a *generous* rivalship." The imputation here conveyed, or as the Lancet is disposed to term it in *elegant* phrase, "the specialty in medical partisanship here assigned," seems actually to have frightened our contemporary. He evinces as much sensibility in regard to it, as if it were a matter of life and death, though, what there is in the imputation to excite so much feeling, is beyond our ken. Is it so heinous a matter for a journal to be devoted to the interests of a school of medicine, and to advance those interests by all *honorable* means? Is it inconsistent with a proper independence? does it presuppose, or involve in any way, a sacrifice of principle? The sort of holy horror with which he views the bare idea of any connection with the Medical College of our sister State, is calculated to excite suspicion; it might easily lead a stranger to suppose there was something wrong about that institution, either that its interests were totally opposed to those *general* ones which, in his immense liberality, the editor of the Lancet has taken under his especial guardianship, or that there was some

upas-like influence, some pestilential atmosphere about the building that was absolutely fatal to every thing so unfortunate as to be in its neighborhood. Yet we cannot but reflect that once upon a time we dwelt within its shadow, almost under its very eaves; that each morning its red front glared on our half-opened eyes, and each evening its burnished windows greeted our backward gaze; and, forsooth, the grass waved as luxuriantly, playing with the iron railing that encloses it, and the trees that adorn its front tossed their vigorous boughs and danced as joyously in the passing breeze, as in the green fields and forests beyond. Not being disposed, however, to assign the Lancet a position that it does not wish to occupy, we correct the error into which we inadvertently fell, and give the editor's disclaimer in his own language, viz. "The Medical College of Ohio does not require our aid, and if it did, we do not feel competent to render such service." As we cannot question the truth of the former clause, so we certainly shall not doubt that of the latter.

The editor's fancy is not only distempered, raising up "gorgons and chimeras dire," but his mental vision is likewise morbidly acute. He is not only able to see as far into a mill-stone as the man that picks it, but he can actually see further; for he has found in the editorial referred to, a censure on all attempts to establish new medical journals! This singular *presbyopia* might be explained by supposing some votary of the magic-like art of mesmerism had been practising on his delicate susceptibility, if it did not evince a degree of perspicacity, a mental clear-sightedness to which the phenomena of *clair-voyance* afford no sort of parallel. He has succeeded so well in exposing what he denominates our "partial and erroneous view of the subject," that we dislike to disturb his self-complacency by showing that such "view" never had any existence save in his own diseased imagination. We hope, however, that upon "sober second thought," he will discover we have somewhat more of liberality and of magnanimity than he now gives us credit for.

The Lancet has a good deal to say about *parties* and *party feuds, party interests, partisan feelings, &c. &c.*, all which *may* be very fine, but, "as we understand it," is about the merest twattle imaginable, much better suited to the columns of some obscure political sheet, than to the pages of a medical journal professing and aiming to be respectable. It is a common observation, too, that those who are *loudest* in their condemnation of such things, who apparently hold them in the *most* abhorrence, are ever the first to submit to their sway.

We do not mean to say at all that such is the case in the present instance; we would fain believe it is not, and are willing to think our contemporary honest in his professions; but we beg him to bear the fact in mind, in his future lucubrations, to the end that no such impression of him be entertained. C.

FALL COURSE OF LECTURES ON ANATOMY AND OPERATIVE SURGERY.

We would remind our friends abroad, that Professors Cobb and Gross will deliver a course of lectures on the above branches of Medical Science, in the Louisville Medical Institute, commencing the 21st of September, and terminating on the last Saturday of October. It would be needless to set forth the benefit to be derived from these lectures, which will embrace visceral and surgical anatomy, with operations on the dead subject. Too little attention is paid to these subjects, more especially to operative surgery, and yet they are as indispensable to one that aspires to be an accomplished surgeon, as are *a, b, c* and *syllabication* to one that wishes to make a scholar. We hope the young men of the west will avail themselves of this opportunity, and be in early attendance on these lectures. C.

PRIVATE MEDICAL INSTRUCTION.

We owe an apology to Drs. Warder and Dennis, of Cincinnati, for having neglected to notice their circular, received some time since, announcing that they have associated themselves for the purpose of giving instruction in the various departments of Medicine and Surgery. The term commences on the first of March and continues one year; the mode of instruction will consist of daily examinations, interspersed with lectures and familiar demonstrations. During the winter especial reference is had to the current lectures in the Medical College of Ohio. From an intimate knowledge of these gentlemen, we do not hesitate to recommend them to favorable notice. The former is professor of Chemistry in Cincinnati College, and both are enthusiastically devoted to their profession. The course of study proposed, though a very extended one, will be faithfully carried out by them. C.

THE

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ART. I.—*Observations on the epidemic Yellow Fever of Natchez, and of the South-west.* By JOHN W. MONETTE, M. D., of, Washington, Mississippi.

(*Concluded.*)

Admitting the correctness of the positions which we have assumed, the question which naturally presents itself is this:

How shall New Orleans and the towns on the lower Mississippi be protected from EPIDEMIC *yellow fever?*

We answer, they may be protected from devastating *epidemics* by judicious quarantine regulations, properly enforced, near the mouth of the Mississippi, or at any proper distance below the city. These regulations should have for their object the prevention of any direct intercourse with Vera Cruz, and Havana, or any other West India port, during the time that such port is the seat of epidemic yellow fever. This would necessarily exclude shipping from Vera Cruz generally from and after the first of June; and from Havana, from

and after the first of July, varying according to the periods at which those ports become imminently infected. This prohibition should be enforced for two months or more, until the epidemic subsides in such ports.

The quarantine regulations should also embrace some provision for reducing the number of transient, unacclimated strangers or northerners, from and after the middle of July, and also for prohibiting the introduction of foreign emigrants into the city between the months of June and October; and, when yellow fever is virulent in the West Indies, the prohibition should be enforced until the last day of October. The danger of any epidemic is always augmented by the influx of northern foreigners during its prevalence. This is well known in New Orleans; and the city press does not fail to warn them of their danger, and vainly to deprecate their arrival.* The civil authorities should coerce their exclusion, if necessary, during an epidemic, or during the apprehension of one. An epidemic cannot become virulent, and extend its influence rapidly among the resident population, without the aid of a large number of northern strangers and foreigners, who are unacclimated; and during an epidemic they are the fuel which keeps up the fire.

If the disease in its epidemic form can be excluded from New Orleans, every interior town in the south-west, having a direct commercial trade with, and dependence upon that city through the lower Mississippi, will most assuredly remain free from this pestilence; and their citizens may rest secure from the apprehension of danger from that quarter. All quarantine restrictions on the river above New Orleans will be unne-

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* The following extract from the Louisiana Advertiser, of October, 1839, will show the general feeling on this subject, viz.:

"*Food for yellow fever.*—The barque Eleanor, which arrived yesterday morning from Havre, brought to the city sixty steerage passengers, chiefly Germans: they are full-blooded, fresh, and healthy—*inviting subjects for yellow fever*. We looked on them as among the *doomed;* and sorrowed to think that such robust and strong-constituted individuals, should be grappled by so fierce an enemy."

cessary; and the interior commerce of the city, with the valley of the Mississippi, will be constant and uninterrupted.

This brings us to speak more particularly on the subject of quarantine regulations, adapted for the protection of New Orleans and other towns on the lower Mississippi.

QUARANTINE.

There are in all countries, and especially in commercial cities, men whose pecuniary interests conflict with wholesome regulations for the welfare of the community at large; men whose feelings and views of things mislead their judgments relative to the general good of society. In nothing is this principle more clearly elucidated, than in the opposition usually made to all temporary restriction upon commerce, by sanatory quarantine regulations.

Notwithstanding the apparent efforts, sustained by the commercial interests, and the weight of medical authority in the United States, to abolish all quarantine regulations designed for the protection of our commercial ports, the facts are continually staring us in the face, that *there are certain diseases*, peculiar to tropical ports, or to certain cities and regions of the civilised world, which are more or less communicable from one person to another; and which, when epidemic in any city, may, by a favorable combination of circumstances, which are not well understood, be communicated and disseminated in other towns and ports, which were previously free from any such disease. The two most prominent diseases of this character, are the *plague* of the eastern continent, and the *yellow fever* of the western, not inaptly termed the western plague. These diseases, when communicated from one port to another, are supposed to be imported solely through the agency of ships and commercial intercourse. When one port or city is more favored by nature than another, either in its location, latitude, or any other circumstance, which renders it exempt from diseases which are endemic in others, it becomes the natural right of the inhabitants, and the special duty of the municipal

authorities of such favored port or city, to protect themselves against all the possibilities of introducing a pestilential disease.

As such diseases are caused by some invisible, uniform, and mysterious influence, which manifests itself only in those persons who are exposed to its action; and as the same disease, when produced in a healthy place, is the result of a similar exposure to the influence of a portion of the same atmospheric agent, which may have been, and doubtless has often been carried from infected ports, in ships and other articles freighted in them, it is the dictate of reason and prudence, to *interdict* temporarily, that kind of intercourse with such infected port, which affords the greatest probability of introducing the aerial poison. This temporary interdiction of commercial intercourse with cities which are the seats of epidemic pestilential diseases, has been denominated a quarantine restriction.

The older quarantine regulations, from some superstitious veneration for the number forty, required vessels from such infected ports to remain out of port for forty days: hence the name. Such restrictions, under one form or another, have been enforced from the earliest history of the commercial world; and the world, by common consent, has approved the same. But with the nineteenth century chiefly, and chiefly in the United States, the doctrine has sprung up, and has been propagated, which teaches that the light of science and the march of intellect, at this late day, deem it expedient to expose populous cities to the ravages of pestilence, rather than impose any wholesome restrictions upon the interests of the few, to avert such calamities.

Such regulations have always been opposed by the commercial interests in any port, because it does not ostensibly administer to the pecuniary advantage of that class of individuals. At length many of the learned of the medical profession have been brought over to sustain this opposition, which, at this time, has become somewhat *popular* in the United States. This circumstance alone is calculated to draw a formidable array of learning and talent against any cause, however sound its positions.

Besides the professions of philanthrophy, benevolence, charity and humanity, as the motives which prompt the opposition to judicious quarantine restrictions, many have taken the ground that the disease—the pestilence to be excluded by such means, is *endemic in our ports;* the product of local causes which abound in all our towns, independent of, and unconnected with foreign intercourse: and that consequently it is not to be excluded by such restrictions against foreign importations. Through the name and influence of Dr. Rush and his coadjutors, backed by the allies of the commercial and importing classes, this salutary protection to the population of some of our most important seaports, had well nigh been abolished. Yet they have passed from the stage of action, and their influence, losing its power over the judgments of men, ceases to mislead on this important subject.

New York, Philadelphia and Baltimore, as if for the purpose of testing the doctrine of foreign importation of yellow fever, at the expense of thousands of their citizens, refused to adopt any decided restrictions on the West India commerce until about the year 1822, when the experiment had been fully made. Since that time, the counterpart, and by far the most salutary part of the experiment, has been in operation.

Since the year 1822, the quarantine restrictions enforced at New York and Philadelphia have protected those ports, and for twenty years they have escaped all pestilent epidemics. Yet before these restrictions were enforced, they had been liable to these visitations, no less than six times in 20 years, from 1790 to 1810.

The southern ports of Charleston, Savannah, Mobile, and New Orleans, where quarantine regulations are discarded, are almost annually ravaged by this West India plague. Will not the same regulations, strictly enforced in these southern ports, be followed by the same salutary results?

It will be remembered that we have already set forth the manner in which vessels and steamboats become infected, and instrumental in the introduction of yellow fever into a healthy port, to which we refer the reader.

How should a quarantine be enforced below New Orleans? We answer, 1st. establish a quarantine ground near Fort Jackson or Fort St. Philip, which shall effectually command the river. From the time that yellow fever has assumed its epidemic form in Vera Cruz, Havana, or any other place, cause any and every vessel from such port, to remain below at the quarantine ground: there let her cargo be discharged, and properly ventilated for 72 hours, at least, day and night, before it is allowed to be taken up to the city: the importing vessel should not enter the port of the city until after frost. At the same time, vessels from healthy ports, which have not touched at any infected port, might be permitted to ascend and discharge their freight at the wharves of the city—provided their crews are perfectly free from disease.

2. Erect a large, airy, brick hospital, at the quarantine ground, for the reception of seamen and others, laboring under yellow-fever, from vessels detained. The buildings should be comprised in an extended row of one-story houses, with rooms about 20 feet square; each room to accommodate four patients.

3. Prohibit, during the quarantine, the entrance of foreign emigrants, or strangers who have touched at any infected port within ten days previously.

4. The Charity hospital, for the reception of the indigent sick of the city, should be located in the suburbs, at least half a mile from the populous part of the city.

It can be well established that whenever yellow fever has prevailed in any port of the United States or in any port of Europe, it was always at a time when a direct intercourse had been kept up between such port and the West Indies where yellow fever is endemic. Whenever it has been epidemic in Lisbon, Cadiz, or Gibralter, it was immediately preceded by an unusual intercourse from the West Indies to those ports. This was true, especially, from 1804 to 1814, when large armaments and fleets were constantly arriving from Havana, Vera Cruz, and other tropical ports, during the European wars with Bonaparte.

The same is true of ports in the United States. We have

already shown that yellow fever is not epidemic in them except when the intercourse with the West Indies is direct and uninterrupted. This point can be established by a reference to every instance in which it has prevailed as an epidemic in our cities. Dr. Hosack observes, that "during the years in which our commerce with the West India ports was interdicted by the embargo (before the year 1812), and during the subsequent years of war with Great Britain, when our communication with her possessions in the West Indies, and other West India ports, was entirely suspended, *the cities and towns along our whole coast were exempt from the fever of the tropics.* A recurrence to meteorological observations, for that period, will show that the mercury frequently ranged higher, than in those years when the yellow fever prevailed in our cities; and when the general constitution of the atmosphere, which was favorable to the generation of this malignant form of fever, provided domestic causes could engender it, pervaded our country as much as at other times, when yellow fever had prevailed extensively. The local causes in our sea-ports, such as exhalations from our ships, market places, privies, and new-made grounds, remained the same, and in some cases worse than when the yellow fever had prevailed in other years. Yet, unfortunately for the doctrine of domestic origin of yellow fever, the health of our cities remained undisturbed by the deadly visitor."* The same was true of all our ports during the war of the revolution, from 1776 to 1785. But so soon as peace was confirmed, and a brisk trade was carried on with the West Indies, the yellow fever began to prevail in all our most important ports, from 1790; and it prevailed extensively in some of the principal ports, nearly every other year, until the embargo in 1808 and the war of 1812 so interrupted the commercial intercourse with the West Indies, that epidemic yellow fever was not known in the United States until the year 1817, when a constant commercial intercourse with the West Indies had become again established.

* See Hosack's Medical Police.

Since that time the intercourse with tropical America has been uninterrupted, and yellow fever as an epidemic has not failed to visit our most important ports, almost every other summer, up to this time.

The great practical error and impolicy of the *domestic origin* of the yellow fever, is this: viz. it leads people to disregard the proper precautions against its dissemination through a dense population; it indirectly encourages its diffusion among a healthy population, by encouraging those habits of careless intercourse by which the limits of the infection is certainly extended; while every measure which tends to circumscribe its limits are neglected or disregarded. Under the belief that it cannot be imported, or even communicated, directly or indirectly (because it is local and indigenous in its origin), many persons are induced to expose themselves to infected ships, houses, and the infected districts of a city, and thereby become instrumental in carrying the disease in their own persons into healthy parts of the cities and towns. This is often the case with persons who have no definite ideas of any of the laws of infection and contagion, and are often blindly impressed with the belief that the disease is "not catching." It leads also to a culpable carelessness with the sick, by indirectly encouraging the accumulation of the morbid effluvia in their rooms, and the accumulation of morbid secretions about their beds and chambers; for, as is very naturally inferred, if no effluvia or morbific exhalation is given off by the patient, why the necessity of cleanliness and ventilation? It encourages a disregard of all sanatory cordons which may be established to cut off the intercourse of the healthy population from the infected vessels, houses, or districts. It is to be regretted that medical men, the proper conservators of health, should be foremost in inculcating doctrines which tend indirectly to lead on these malignant epidemics into our cities.

Medical men are often led by theory, or the force of education, to disregard important facts in these epidemics; or to bend them to suit a preconceived theory: but the intelligent mass of the people have neither theory nor the bias of educa-

tion to mislead them, and they generally judge correctly upon the facts which present themselves. It is a well known fact that the mass of the intelligent population in our southern ports, where yellow fever is often epidemic, are involuntarily led to the conviction, that it is an imported, and, under some circumstances, a communicable disease. They know that ships and steamboats do sometimes become strongly infected while the resident population is healthy. This conviction is produced in their minds, without any desire to sustain any theory, or from any pride of opinion. As the Abbe Du Pratt has remarked, "there is often *a blind impulse in public opinion which arrives by a more speedy and certain method at the truth*, than all the reasonings of philosophy."*

Those who oppose all quarantine restrictions, urge the plea that the commercial interests are deeply embarrassed, and the general prosperity of the cities interrupted. These we conceive to be false positions. We contend, on the contrary, that the health, prosperity and pecuniary interests of all classes are promoted by proper quarantine regulations. For example, let us take New Orleans and Natchez, which might be protected from yellow fever epidemics by judicious quarantine restrictions.

When Natchez is the seat of epidemic yellow fever, the whole commercial business of the place may be said to be prostrated for that time; all mechanical business and trade is stagnant for two or three months, and the ordinary income of at least five hundred families† is suspended, and their daily expenses increased fourforld, by flight, sickness, abandonment of property and new expenses incurred by procuring a temporary residence out of the city. In this we have not included the permanent loss which the community sustains in the death of two or three hundred of the most enterprising and useful citizens, at each visitation, exclusive of an incalculable amount of suffering, privation and bereavement which are but

* See Townsend on Yellow Fever of New York, p. 62.

† The entire population of Natchez during the summer is 3500.

partially indicated by the emblems and weeds of mourning which shroud the community for months afterwards. All this is avoided and escaped during the summers when yellow fever does not visit the city; and with incalculable advantage to the dependent classes.

Whatever may be the foreign trade of New Orleans, it is almost entirely suspended during the prevalence of an epidemic in the city; and, what is of far more importance to the prosperity of the city, the whole inland commerce of the valley of the Mississippi, with a population of more than four millions, is likewise suspended. Besides this, nearly five thousand of the resident population annually leave the city in anticipated fear of an epidemic, and an equal number leave on the certain appearance of the unwelcome visitor. In addition to this, the city suffers a permanent loss by the death of not less than two thousand or twenty-five hundred souls, during each epidemic; and among this number may be reckoned about five hundred enterprising and useful citizens forever lost. We might add, without taking into the estimate the amount of mental and physical suffering, that the charge, responsibility, and expense of providing for, nursing, and interring more than one thousand destitute strangers, is thrown upon the city at each epidemic visitation.

All this may be avoided by excluding yellow fever from the city; and the enforcement of a judicious quarantine near the Balize, and on any other commercial avenue for three months at most, during the prevalence of yellow fever in the West Indies, affords the only prospect of accomplishing this desirable result. New Orleans may be, and at length will be released from this annual pest, if her citizens will it to be done.

The plea of humanity, of philanthrophy, in this case so often preferred, as an argument against quarantine restrictions, with apparent sincerity, is perfectly gratuitous. If we admit the position that yellow fever is local in its origin, and has no tendency to spread among a dense population, and that the multiplication of cases does not tend still further to contaminate the air; and that quarantine restrictions encourage

the abandonment and entire neglect of the sick, it then would appear a very plausible argument, *ad captandum*. But denying all these assumptions, we contend that we are entitled to the whole benefit of the argument on the score of humanity and philanthrophy. We urge that it is not local and that it can be excluded from our ports; and by so doing we should be the means of preserving the lives of thousands of persons annually in the United States; and we should in like manner protect our citizens from all the privations and sufferings incident to these epidemics, and the concomitant stagnation of every species of trade and business upon which the poor depend for daily subsistence. We *prevent* the misery and wretchedness which others would in vain attempt to *mitigate*, after they have aided in bringing it about.

We have devoted none of our remarks to the treatment of this disease. All the experience of the medical profession in the United States, for fifty years, and all the researches of science, and all the *post mortem* examinations of organs and tissues, have tended to throw no light on the treatment. Under all modes of treatment, consistent with the various theories framed for its cure, the disease in its most malignant type sweeps off into the grave at least one half of its victims, and with as much celerity and certainty now as it did when first introduced into our ports fifty years ago. The milder cases which occur in all these epidemics, will recover under any judicious mode of treatment much more readily than severe cases of common remittent fever. Many of the mildest cases will recover almost without any medical treatment. But the severe cases generally terminate in death, unchanged by any mode of therapeutic treatment. Hence we doubt not that it is much more important to the interests of our cities and ports, to direct attention to *preventing* these epidemics, than to discover the best modes of cure after they have been invited among us.

CONCLUSIONS.

If the views advanced in the preceding pages be correct, we are sustained in the following as legitimate deductions:

I. That the miasm or gaseous matter which is essential to the production of epidemic yellow fever, is generated only while the extreme temperature in the shade is at least up to 88° of Fahrenheit; and that so long as there is sufficient agitation and change of the air by winds, it will not accumulate in sufficient quantity to produce yellow fever as an epidemic: that when sufficient miasm is produced and accumulated, the malarious combination, which likewise requires several days of calm, sultry weather, will proceed at a still lower temperature.

II. That the miasm of yellow fever, is, *per se*, in a pure atmosphere, probably innoxious; but acquires active morbific properties by combining with sultry, hot air, which has been exhausted by respiration, and charged with human exhalations: and that it then becomes *malaria*, or infectious air, which is an active predisposing cause, as well as a proper "*nidus*" for the reception and evolution of the infection of yellow fever.

III. That this malarious condition of the local atmosphere of any city, or portion of a city, may be so concentrated as to produce a strong predisposition to yellow fever in many of the inhabitants, without actual disease, until after a few cases have been excited into action by highly exciting causes, when *infection* is generated, and speedily the malarious district becomes the infected district: which result would have been prevented by a storm or change of weather previous to those cases.

IV. That when the malarious combination is sufficiently concentrated for this purpose, a large quantity of *infected air*, brought from an infected district, or a large number of cases of yellow fever introduced from another point, will convert that malaria into infected air, and produce an epidemic likewise.

V. That consequently, although yellow fever may to a certain extent be called a disease of local origin, and depend upon a local atmospheric constitution, or contamination, for its extension, yet it may, under some circumstances, be carried from one city to another, and there propagated.

VI. That accordingly epidemic yellow fever may be averted sometimes by one or all of the following measures, enforced at a time, when, according to the principles herein set forth, the malaria is forming rapidly; viz:

1. By a dispersion of the greater portion of the citizens to the country.

2. By removing from the city, and especially from the districts usually infected, all *strangers*, or those who have not become acclimated by a residence of two or three years, and who would of course be the first attacked.

3. By prohibiting the introduction from foreign places of infected air, or *fomites*, or patients laboring under yellow fever during the prevalence of malarious accumulations.

VII. That malaria accumulates most in those parts of cities and towns, where the population is most crowded, and where, from the situation of the ground, or the nature of the buildings and enclosures, the local atmosphere becomes most stagnant; and such accumulations of malaria in the United States take place chiefly in the months of July, August, and September.

VIII. That when yellow fever rages like a pestilence in any city or town, the suburbs, the immediate vicinity, and the whole surrounding country, are entirely free from such infected air, and from all similar disease. Of course the epidemic yellow fever cannot be the result, solely, of "a general atmospheric constitution"—but is the result of some peculiar local contamination of the city atmosphere.

IX. That this local contamination, or infection, *is not* the product of the city filth usually found in streets, alleys, and sewers, and about the wharves; nor is it the product of the decomposition of animal or vegetable matters, under any circumstances.

X. That when this contamination or infection is once formed in the local atmosphere, none of the disinfecting agents heretofore used, have any virtue in neutralising the infection, or in arresting the progress of an epidemic. Cold, or a reduced temperature, is the only known destroyer of the infection of yellow fever.

XI. When an epidemic has once begun, the only safety to the unacclimated is speedy flight to the country, where the air is pure. When the population of an infected town has fled in great numbers to any healthy town, carrying with them large quantities of clothes, beds, bedding, blankets and porous articles of merchandise, there is great danger to be apprehended, lest the fomites should there generate a new local infection equally virulent with that from which they had escaped. These articles, when infected, are liable to become the most virulent sources of infection, and are purified only by frost or cold weather.

XII. That all steamboats, flat-boats and vessels which remain at the wharves for many days, in an infected district, discharging and receiving freight from the infected district, do often become as thoroughly infected as the houses in that part of the town, and finally become as capable of imparting the disease to unacclimated strangers, as the air in the city houses would, under similar circumstances.

XIII. Consequently such vessels from an infected port, should be prohibited from lying at the wharves of a healthy town, and from discharging their freight or sending into such town yellow fever patients, or in any wise holding intercourse with the population of that town during the months of July, August or September.

XIV. That the cause of humanity, both in relation to the sick and the well, and especially to the latter, requires that all hospitals provided for the reception of boatmen, sailors, and all indigent poor, during an epidemic yellow fever, should be located beyond the limits of any city or town, and that the free and promiscuous intercourse of the citizens with such hospital should be interdicted.

XV. That all empty and unoccupied flat-boats at the wharves should be removed early in summer, as they contain a large amount of stagnant atmosphere constantly acted on by the intense rays of the sun, and they may thus become reservoirs of what we have termed *miasm* and *malaria.*

The following *facts* relative to yellow fever in its *epidemic form,* appear to be well established by experience and observation, especially as regards the United States.

1. Foreigners and northern strangers are often attacked with yellow fever in the southern ports of the United States, as well as in the West Indies, when the whole resident or acclimated population is healthy; and the former are the principal victims during any yellow-fever epidemic.*

2. It is extremely rare that any person is attacked a second time by yellow fever. The first attack produces such a change in the constitution that those who recover are generally forever afterwards exempt from its attack, while they reside in a southern latitude.†

3. In the most virulent epidemics, at least *one half* of those attacked by yellow fever die; when less than that proportion is reported, we may believe many cases have been enumerated as yellow fever which *are not yellow fever;* or that the disease assumes an uncommonly mild form.

4. When an infected district is once formed, the infection spreads slowly and gradually from one or more points, or radiating centres, until it pervades the principal parts of such city or port. In its progress it advances steadily, regardless of promenades, burying-grounds, compact or open squares; over sewers, cist-pools, clean or filthy streets, and in wet or dry weather; it pursues its retreating victims to their farthest retreat, provided they have contracted the seeds of the disease, or breathed the concentrated infection.

5. During an epidemic the virulence of the infection is *more active by day* than at night, and exposure in the infected dis-

* See Townsend on the Yellow Fever of New York, pp. 249–252.
† Vide Ibidem, pp. 247–9.

trict is more dangerous at noonday than at night. Night-watches are more exempt, *ceteris paribus*, than the day-watch, from the attacks of the disease.*

6. Common city filth *has no agency whatever* in generating yellow fever epidemics; on the contrary, it is calculated to absorb or neutralise the infection.† Scavengers and grave-diggers have always been more exempt from attacks of this disease than others.

7. Lime as a *prophylactic*, or a *disinfecting agent*, is *useless if not prejudicial.* Those engaged in spreading lime in cities have been more frequently attacked by the disease than the scavengers. In New York in 1822, the yellow fever spread much more rapidly in Lombardy and Cheapside streets, *after* lime had been spread in them, than previously.

8. Next to blankets, feather-beds, and other similar porous articles, *wood*, such as plank, boxes, ships, wooden houses, &c., retain the greatest amount of *latent infection.* Hence yellow fever prevails most malignantly among crowded wooden buildings,‡ board-fences, temporary sheds, wharf-boats, &c., all of which retain the infection until it is neutralised by cold or frost.

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* See Townsend on Yellow Fever of New York, pp. 252, 253.

† Idem, p. 116.—See Dr. Rush's Inquiries.

‡ The following fact stated by Dr. Bayley, in relation to the tenacity with which fætor from putrescent animal matters adheres to the wood and timbers of ships, will in some degree aid in illustrating the tenacity with which yellow-fever infection adheres to wooden houses, boxes, &c., viz:

"I have often noticed that those vessels loaded with hides and jerked beef which was partly damaged, and where the timbers and ceiling of the vessels became tainted, especially in the hold, the offensive smell has often been removed for the time, by being well ventilated and scrubbed with water:—but afterwards, upon the application of whitewash to the planks, *the offensive smell has been reproduced to a much greater degree than it was at first.* A second white-washing with lime, after the foul smell had again disappeared, again *renewed the putrid effluvia.* This would be produced in a decreasing degree for several times until the vessel was entirely purified." See Townsend on Yellow Fever of New York, pp. 94, 285–321.

This effect was produced by the chemical action of the lime, disengaging the putrid matter absorbed by the wood in a latent state. In the case of the brig Enterprise before cited, infection seems to have been absorbed by her timbers in the same way. The same action may be exerted upon the infection of yellow fever.

9. That yellow fever is a peculiar disease; a fever of only one paroxysm, varying from one to three days before the remission and collapse; and that it is radically different in its character from the whole family of remittent and intermittent fevers.

YELLOW FEVER HAS BEEN EPIDEMIC IN THE FOLLOWING TOWNS AND PORTS OF THE UNITED STATES

1. Charleston, S. C. This is one of the oldest commercial ports in the United States, and has at all times had an extensive commercial intercourse with the West Indies: and it has likewise been subject to epidemic yellow fever repeatedly during its commercial prosperity.

The *first epidemic* was in the year 1700, again in the years 1703, 1728, 1745, 1748, 1753, 1755. The next was in 1793. From the year 1793 to that of 1807 inclusive, this disease prevailed in Charleston almost every alternate year, either severely or partially.* From that time until the restoration of commerce, after the peace of 1815, this city, as well as all other ports of the United States, was exempt from this disease. In 1817 the city was severely visited again; and from that time up to the year 1839, a period of 22 years of active and uninterrupted commerce, it has been epidemic in Charleston nine times, or nearly every alternate year. In this city the disease, always appearing among the vessels in port, and in unacclimated persons, is known chiefly by the name of the *stranger's fever.*

2. Philadelphia. This is one of the oldest commercial ports in the Union, but being in a more northern latitude was less liable to epidemic yellow fever than Charleston; yet it was occasionally visited by this disease up to the year 1762. From that time, during all the difficulties and interruptions of foreign commerce, until the close of the war of independence.

* See Med. Repos., old series, vol. ii, pp. 234, 235, and vol. iv, p. 100. See Dr. Simonds on Yellow Fever of Charleston.

the city was free from this disease. After the peace of 1783 the foreign commerce gradually revived, and having increased rapidly, Philadelphia became an important port, as the seat of the Federal Government. The consequence was, that yellow fever was introduced every alternate year, until the year 1808, when commerce was again interrupted by new difficulties with England and other European belligerants. This interruption, with the war which followed, gave Philadelphia another exemption for ten years. Since the year 1822, the city, having adopted the policy of New York in prohibiting infected vessels from the port, has enjoyed an exemption from these epidemics, and her commercial interests have been benefited rather than injured.

We may remark, that during the revolutionary war, the autumn of 1778 was *unusually sickly* along the whole Atlantic seaboard, the summer "*being unusually hot and sultry*": but there was no yellow fever, for the want of foreign infection, the general character of the diseases being of the "intermittent" type.* Yet so soon as commerce with the West Indies was fully revived, the diseases in the seaports dropped the "intermittent" character, and assumed that of epidemic yellow fever. During this time the British army and navy in the West Indies were annually ravaged by this pestilence which prevailed in nearly all the West India ports;† while the inhabitants of the interior were exempt.

3. New York, lat. 40° 43′ north. This city has been gradually increasing in point of population and commerce for the last fifty years, during which it has more than doubled its commerce and population. In a latitude almost too far north for epidemic yellow fever, the city has never been so extensively ravaged by this disease as the more southern ports. Yet it has suffered to a considerable extent, several times previous to the year 1808. After the great expansion of commerce after the late war, yellow fever was occasionally intro-

* See Med. Repos., old series, vol. ii, p. 364.
† Coxe's Med. Museum, 1805, vol. i, p. 184.

duced by vessels from the West Indies. But it did not spread epidemically except in 1819 and 1822, the latter being much the most fatal of any previous year.*

Since the year 1822, a period of nearly twenty years, a judicious quarantine has protected the city from epidemic yellow fever, although the commerce of the city during that time has been more extensive than it ever was previously; while Charleston, Savannah, Mobile, and New Orleans, without any quarantine restrictions, have suffered severely.

4. Savannah. This also is one of the oldest ports of entry. From a very early period it has been occasionally visited by epidemic yellow fever, when it had been prevailing extensively in the West Indies. From the year 1808 to 1817, during the interruption of commerce, it was exempt. Since that time it rarely escapes when Charleston, Mobile and New Orleans are visited. No quarantine regulations have been adopted in this city.

5. St. Augustine & Pensacola, under the Spanish occupancy, were proverbial for their salubrity, and yellow fever was unknown in them as an epidemic until the Floridas fell under the jurisdiction of the United States on the 17th of June, 1821.† As the treaty of cession had been made several months previously, the emigrants from the United States were pouring into St. Augustine early in the summer of 1821, while the Spaniards were departing as fast as possible for Cuba. Thus a constant intercourse was kept up between Cuba and St. Augustine, by numerous vessels which were transporting the Spaniards and their effects to Havana. These vessels on their return were chiefly freighted with tropical fruits, which were the means, no doubt, of producing an epidemic bilious fever, while some persons contracted yellow fever in the vessels direct from Havana. The following year, 1822, the American emigrants began to crowd into Pensacola; while transports were constantly passing and repassing to Havana, in removing the

*See Townsend on Yellow Fever of New York, passim A. D. 1822—also N. Y. Med. Repos., vol. ii, pp. 315 and 316.

† William's Florida, p. 207, &c.

persons and effects of the loyal Spaniards. The result was the introduction of a malignant epidemic, which spread rapidly among the unacclimated and promiscuous population who remained in the place. Again, in the autumn of 1825, the population was composed chiefly of recent northern emigrants, who were pouring in daily; and a brisk trade having sprung up with the port of Havana, the yellow fever was again introduced with great mortality. Since that period Mobile and other ports in the Territory of Florida, having withdrawn the trade from Pensacola, have likewise taken away the liability to frequent yellow fever epidemics; and for many years past, Pensacola has rarely suffered from yellow fever, although cases have been occasionally introduced in the national vessels of the United States and other powers.

6. New Orleans. Site on adhesive alluvial earth, or mud, 10½ feet above tide level, in lat. 29° 57′ north. This city has carried on a constant commercial intercourse with Havana and other West India ports, for more than seventy-five years, including the Spanish régime previous to 1803. During this long period, we are not able to ascertain that it has been visited by any malignant epidemic, until it came under the jurisdiction of the United States. Since that time, and especially since the revival of commerce, after the peace of 1815, its population and commerce have increased more rapidly than those of any city in the United States; and the frequency and mortality of yellow fever epidemics have been *pari passu*. In the period of twenty years from 1817 to 1837, the yellow fever has prevailed as an epidemic about nine times; and to such a degree of virulence that in 1819, when the resident summer population was only *thirty-three thousand souls*, the deaths in August were 560 souls, and in September 594. The yellow fever generally, if not invariably, begins its ravages among the shipping and in the population near the wharves.

We infer that any interruption to the commerce of New-Orleans and other Atlantic ports, such as that preceding and during the last war with Great Britain, would again suspend the prevalence of yellow fever in our ports.

For many years after the close of the war of independence, yellow fever was introduced occasionally into some other ports of less note than those we have already named. Thus it was introduced into Baltimore, into Wilmington, Delaware; Port Elizabeth, New Jersey; Norfolk, Virginia; Wilmington, Newbern, and Washington, North Carolina; New London, Connecticut, and Portsmouth, New Hampshire. Yet in most of these places, which were then important ports of entry, the disease manifested itself by only a few cases; and in those only, in most instances, who had had direct communication with infected ships, then lying in port,* which were charged with "foul air," as it was termed.

7. Gibralter and Cadiz, ports of Spain, have been visited by yellow fever. It prevailed in Gibralter in the summer of the years 1804, 1810, and 1813, and at Cadiz in the autumn of the year 1814. In each of these places it was introduced by vessels from the Spanish Main and the West Indies; and especially by the arrival of large fleets and armaments during the prevalence of the European wars against the power of Napoleon. It has also occasionally appeared in other ports of southern Europe at divers times between the years 1800 and 1815; but there are the strongest reasons to convince us that the disease was introduced by vessels and transports from the West Indies, during the wars which devastated Europe and the West Indies within that period. The troops and marines were those who suffered first and most severely.

Art. II.—*Facts and Conjectures on the Trembles and Milk-sickness.* In a letter from Dr. G. B. Taylor of Morganfield, Kentucky, to Dr. W. L. Sutton, of Georgetown in this State; communicated by the latter with some remarks.

* * * The subject of your letter is one on which I have made many inquiries, and in every neighborhood within

* Med. Repos., vol. ii, p. 117.

my reach; especially since the death of Wm. Hopgood, with whom you were well acquainted: he died at Highland about three years since. As he was a young man of close observation, and possessed more than common discrimination, I was disposed to place some confidence in his opinion of the producing cause of his disease; and I never saw a man more confident of any thing being the cause of his sickness, than he was that his was produced by mineral water, which he drank in the Highland hills, and at the very place where most of our cattle and horses have died. I did not see Hopgood until three or four days after his attack. When I visited him first, he was salivated, and his bowels had been freely acted on by oil, &c. He complained of feeling very tired, and although naturally neat, would not turn his head to spit, so as to avoid spitting on his bed. The action of the heart was so strong, that you could see the whole system shake at every pulsation. The pulse was slow and strong, not more than fifty strokes to the minute after I saw him; and I could count the strokes by putting my hand on the top of his head. I bled him freely three times, and he grew worse after each bleeding. He was disposed to faint when bled, and upon recovering from syncope, would vomit, and in a short time after, the pulse would be as strong as ever. I think he died upon the second day after I saw him. * * * * * I have been in the habit of making inquiry of intelligent persons in the different neighborhoods, where they are liable to the disease; and I find the same mineral exists in every neighborhood—one in Union, two in Henderson, and three in Daviess counties. In all those places there are sweet springs, the water running from thin strata of stone coal, which contains so much sulphate of iron, that the smiths cannot use it without overhauling and throwing out the mineral. All those places are in the immediate neighborhood of sugar-tree hills, and I think an unusual growth of poison oak. I am satisfied myself that the disease is produced by the mineral, and that it contains a portion of arsenic. There is also some petroleum on the surface of the water at these sweet springs; and stock resort to them as *licks*, for want

of salt. * * * * * There is no country I have visited, that contained more poison oak, than that rich section in which you are situated, until it was destroyed by the stock. Horses and cattle feed on it with avidity, wherever it grows, and with impunity. I have just settled in the woods where the poison oak is very abundant. I have no stable as yet, and my riding horse has skinned every poison oak vine I can see, as high as he can reach. There have been several trees cut down with large vines on them, and he has peeled them the whole length, where he could get at them, and devoured all the small branches. I believe that females are not so liable to this disease during lactation, though not entirely exempt; as there was an instance at Leonard's, when he lived on the hill at Highland, of a cow and calf dying about the same time—the cow running in a stalk field, and the calf in a calf lot. There are two distinct vines called poison oak—the five-leaved and three-leaved. Both climb trees, and very much resemble each other. The poison kind has three leaves on the stem and puts out more radicles, attaches itself more firmly to the tree, and bears a small bunch of berries, very much resembling grapes, but smaller and blue. I have known of children eating them twice since I can recollect. It did not prove fatal in either of the cases; but both were very distressing, and neither of the subjects could see for a week or more. And they were certainly affected very differently from the cases of milk-sickness which have come under my observation. The poison oak will not poison all persons, which any farmer will tell you. Some can cut a green vine, and mark their shirts while on them, with impunity. I was confined near a month, when a boy, by doing so, and was as full of blisters all over my body, as ever any person was of pustules from small-pox. I have conversed with several persons who have had the milk-sickness, some at different times. They all say they feel tired constantly, and are incapable of taking much exercise, until they have been freely purged, which generally cures slight attacks. They consider calomel injurious. Those who have been hard at work about the time of attack, rarely recover.

Senna, salts and jalap are considered among the best remedies. I think some of the preparations of iron would be good. Buzzards will not feed on the carcass unless compelled by hunger. I have noticed them sit upon trees around and watch the carcass until it rotted, without breaking the hide. But I have seen them, when they had been pressed by hunger, lying dead around the carcass. Some slowly expanded their wings, on my approach, as if about to fly, but, at the effort to rise, would tumble forward, unable to raise themselves. You know that we, at one time, had a fine pack of dogs. We frequently lost them by their eating carrion in the neighborhood of Highland. Some would come home with the appearance of being foundered, but were never fit for the chase afterwards, as they would take the 'tires' as it is generally termed; and but few horses or oxen are of much use, after having the disease badly. Mr. A. Silver once lived at Highland, and said he could cure his cattle, if he saw them in time, by giving them enough of the decoction of white walnut to purge them. Peter Rives, at one time, was under the impression that arsenic would cure the distemper in his dogs, and was in the habit of giving it freely in that disease. Several were cured of distemper, but they were of no use in the chase, and appeared to be affected precisely in the same manner as those which had the 'tires' from eating cattle that had died from milk-sickness, which is my strongest reason for suspecting arsenic in the mineral. The disease produced by poison oak (so far as has come under my observation) differs so widely from milk-sickness, that I cannot attribute the disease to that vegetable. * * * * *

Fall before last, we suffered very much for stock water, and several persons lost all the cattle they had. They died about these mineral springs in the hills at Highland, and the owners think they did not go into the bottoms. The people in the neighborhood of the disease, attribute it to something that grows in the rich sugar-tree hills exclusively; but we have the same kind of land and growth where there is no disease of the kind. I had forgotten to say that work oxen, regularly fed, are liable to the disease, at the mouth of Highland. If the at-

tack is slight, they will recover entirely by rest—if severe, they are worthless for the yoke afterwards. If the attack is slight, very hard labor will destroy the animal. Hogs and deer are not liable to the disease that I have ever heard of.

ADDITIONS BY DR. SUTTON.

In addition to the above, I will mention that since I commenced this letter, I have conversed with a very respectable lawyer, who has resided at Mill's Point, Ky., and at a place in Tennessee, where also the milk-sickness was common. He says that in both neighborhoods, the people ascribed the origin of the complaint to the water; that the disease was much more common in dry seasons; and that, at such times, certain springs blamed with its production, acquired a taste which was not observable when the water was flush. He gave a number of facts, tending to confirm the general opinion, but which would swell this communication to an unwarrantable length. Upon the whole, they corresponded mainly with those held by Dr. Taylor. Again, I was informed by the widow of a Dr. Rainey, who resided somewhere not far from Covington, and who had a good deal of practice in the disease, that he had under his care a man and his wife (perhaps both died, but of this I am not positive), who had abstained entirely from milk and its products for a long time, for fear of the disease, in whom, nevertheless, it was well marked. Of course I can say nothing as to the accuracy of his observations, or the faithfulness of the report. The lady is quite intelligent, but there are a thousand notions upon the subject; and I cannot say that I am satisfied there is any such specific disease.

February, 1841.

Art. III.—*Wound of the Antrum of Highmore, with destruction of the eye; fragment of knife-blade removed from the antrum two years afterwards.* By W. H. Donne, M. D., of Louisville. Reported by R. S. Wendel, Student of Medicine.

Schuti, a gardener, aged 42 years, a native of Germany, in a rencontre with an athletic man, on the 3d of May, 1840, was struck with a dirk-knife which entered about an inch above the right superciliary arch, passed through the corresponding eyelid, downwards and backwards, evacuating the humors of the eye, and penetrated the antrum. The globe of the eye was divided by a vertical incision, through which the aqueous humor escaped; the iris was extensively detached at the ciliary margin, and could be partially seen through the transparent cornea—its surface being somewhat obscured by small coagula. The hemorrhage was slight, and easily controlled by moderate pressure. The patient complained of intense pain in the temple and cheek of the wounded side, shooting deep into the orbit. Three points of interrupted suture were used to approximate the edges of the divided eye. Lint saturated with laudanum and warm water, constituted the dressing.

May 4th.—Some tumefaction in the eyelid; pulse 110; tongue coated and dry; skin hot; patient has spent a very restless night. Ordered following medicine:

Tart. Emetic, gr. j.
Sulph. Magnesia, ℥ss.

to be dissolved in one-half pint of water, and a table-spoonful to be taken every half-hour, until nausea is induced—after which the interval may be increased.

May 5th.—Bowels freely evacuated; pain less; skin moist; pulse 90 and soft. From this period until the wound healed—the space of three weeks—no constitutional symptoms of an untoward character occurred. The patient, however, contended that a portion of the knife-blade remained in the roof of his mouth. But on the most careful examination no foreign body could be detected.

On the 10th of August, 1842, Mr. Schuti called and requested Dr. Donne to examine his mouth, stating that for six months past he had been annoyed by a rough projecting substance, which some person had informed him was a piece of dead bone, but which he believed to be the point of the knife, that had been driven down into the bone by the violence of the blow. On looking into the mouth a small black speck was discernible about one-half inch from the interval between the first and second molar teeth. The parts adjacent were somewhat tumefied and inflamed. Dr. Donne made several attempts to extract this body with a pair of common dissecting forceps, but found it immovably fixed in the substance of the bone. By dissecting around it with a bistoury down to the palate process of the superior maxillary bone, he was enabled to get a firmer hold, and with a pair of curved tooth forceps, succeeded in removing a fragment of the blade, 1¼ inches in length, and ¾ in. wide at the widest part; the extraction was not effected without considerable violence, and was attended with extreme suffering. The fragment came out with an audible snap which induced those present to suppose at first that it had been broken; but on inspecting its surfaces closely, they were found similarly oxidised and wanting the lustre which a recent fracture would have presented. Upon probing the aperture through which the fragment had been extracted, no other piece could be detected. This opening would scarcely admit the curved probe which Dr. Donne passed into the antrum, in order to satisfy himself that the whole of the foreign body was removed. The next day there was a slight discharge from the aperture, though the patient has suffered very little pain since the operation.

August, 1842.

[This case is certainly of a novel character. The rapidity with which the injury was repaired, and the length of time during which the fragment of the blade remained imbedded in the antrum, without causing much if any trouble to the patient, will at once strike the reader. It adds another to the

many evidences on record of how much a sound constitution can effect, when aided by simple, yet prompt and judicious treatment such as was pursued by Dr. Donne in this instance. We should think it not improbable, however, that the patient may still have some difficulty, as the long sojourn of the broken knife in the antrum must have left the mucous membrane in a condition to take on disease from a slight cause. C.]

Selections from American and Foreign Journals.

Effects of the Times on the Profession.—Strange as it may appear to some, those who are most familiar with the professional interests of the country know very well that the existing hard times affect even the profession of medicine. In the first place, fewer books are published, and consequently, one of the avenues, at least, to science, is partially closed. A disposition to curtail individual expenses in order to meet the world as it presents itself, deranged in its affairs, obliges those who always have been the generous patrons of authors and printers, to withhold subscriptions and purchases, and thus this department of industry and labor, connected with medical science, necessarily suffers. New books elicit new ideas in those who read them. Without their multiplication, therefore, there cannot be that mental activity which is produced by the silent, yet cogent stimulus of new treatises, new theories, or the announcement of important discoveries in medicine, surgery and physiology. Thus, in a measure, we are enabled to explain the comparative paucity of original communications in the journals, which are not so generously supplied by correspondents as they have been heretofore. Instead of reading, reflecting and writing as much as formerly, those who were efficient contributors to the scientific periodicals of this country are compelled to bestow much of their attention upon other objects, which the state of the times renders imperative.

In the course of an extensive tour through an enterprising section of the Union, distinguished alike for the fruitfulness of the soil and the vigor and intellectual energy of the inhabitants, the same complaint was heard that has so long rung in our ears at home. And it was a subject of frequent observation, that no class of men were more desponding, or seemed to feel the pressure more severely, than medical practitioners.

Now at first view it may appear absolutely absurd that the scarcity of money, over the civilized world, should so affect the condition of a physician—as though people could not or would not be sick in adverse as well as in prosperous times. The fact is, the hardness of the times has increased the labors of physicians, but they can get little or nothing for their business. Collections, in the country, cannot be made; and the physician, who of all men is dependent on others for the price of his time, finds an increase of fatigue, responsibility and vexation, without obtaining, in many instances, a decent support for his family. Thus, the hardness of the times directly affects the condition of our professional brethren. We are quite sure that this view of the matter is essentially true; and will explain satisfactorily any apparent lack of interest in the advancement of medical science, with those who were formerly distinguished for their activity in promoting its objects and extending its boundaries. The medical practitioner should remember, however, that he may in the end be a loser by retaining the small pecuniary amount which he has been in the habit of paying for medical books and periodicals.—*Boston Med. and Surg. Jour.*

Scriveners' Spasm cured by Division of Muscles. By Professor Stromeyer.—The disease which may be called scriveners' spasm, and consists of permanent contraction of the long flexor of the thumb, is not very rare. Several cases have been published in Germany by Albers, Heyfelder, and Kopp; and in France by Cazenave and David. The chief characteristic of this affection is an absolute incapability of using the pen, for writing, although the strength and motions of the hand remain unimpaired for all other purposes. Some authors regard it as a species of spasm; others, as depending on paralysis; it is generally permanent, but occasionally appears at intervals, and then is commonly brought on by long continued use of the pen. This affection, though apparently slight, is most obstinate, and resists every method of treatment that has hitherto been employed against it. The following cases, which we abridge considerably, had baffled the skill of Professor Stromeyer, until he had recourse to division of the flexor muscle, which was attended with complete success.

Case I.—J. M., a public writer at Hanover, labored under this disease during two years. A celebrated physician had tried every kind of remedy without avail. Whenever he began

to write, the muscles of the ball of the thumb were seized with spasm, which compelled him to desist, but the spasms did not appear at any other time. M. Stromeyer also tried a variety of means ineffectually, and then divided the small flexor muscles of the thumb. This likewise failed, and the sensibility of the palmar surface of the thumb was destroyed. It was now clear that the action of writing depended upon the long flexor, but the patient refused to permit any more operations to be performed on him.

Case II.—The subject of this case had suffered under the disease for fifteen years, when first seen by M. Stromeyer. The rigidity of the muscles of the ball of the thumb was not, however, present, but the last phalanx of the thumb became suddenly flexed, whenever the patient attempted to write or play on the piano.

The long flexor was not permanently contracted, nor did it impede any other motions of the hand. From the deep situation of the muscle it was not easy to divide it separately. M. Stromeyer bent the first phalanx strongly to a right angle, and at the same time turned the thumb as much out as possible; he then passed a very small, curved tenotome underneath the tendon, and divided it. The sensibility of the thumb was very considerably diminished after the operation, but was restored, on the dorsal aspect the next day, and on the palmar aspect within a fortnight. The natural power of moving the thumb, also, returned at the latter period, and the patient was able to write or play on the piano without the slighest return of the spasm.—*N. Y. Lancet,* from *Arch. Gen. de Med.*

Complete Prolapsus and Separation of the Vagina.—A woman, 25 years of age, who labored under under some slight disorder occasioned by errors of diet, took an emetic on the fourth day, which produced copious vomiting, and relieved her greatly.

A few days afterwards she began to complain of burning pain during micturition, and had some discharge of blood from the vagina, with severe pain in the external organs of generation. On examination, the external parts were found in a state of gangrene; on the following day the patient complained of an unusual feeling about the parts, and it was found that the whole vagina was prolapsed; as it was impossible to return it, antiseptic remedies were merely applied. The exposed parts now sloughed, and were finally completely re-

moved on division of a band which retained them superiorly. The fever and other unfavorable symptoms quickly subsided; to prevent adhesion of the parts, a piece of sponge, moistened with some aromatic decoction, was introduced into the vagina. The vaginal portion of the uterus now became adherent to the upper edge of the vagina, while the lower remained free, and a new canal was formed, being merely somewhat shorter than the original vagina. The woman recovered perfectly, and the functions of the uterus were soon restored.—*Provin. Med. Jour.*, July 2, 1842, from *Orvosi Tár, or the Hungarian Magazine of Health.*

Injury to the Cartilages of the Ribs—Removal of a portion of them, and Cure.—M. L., aged twenty-eight, was admitted into the University College Hospital, February 21, under the care of Mr. Liston. On the night of the 20th, he took six-penny-worth of laudanum for the purpose of self-destruction, but finding no effect from it, he took a dinner-knife, sharpened the point as well as he could on the hearth-stone, put it to his left side, rested the handle on the bed, and fell upon it. Says he tried to pass the knife between the ribs, but could not. There was some bleeding at first, but it soon ceased, and he tried several times to renew the bleeding, by passing the knife into the wound, and moving it about. He was brought into the hospital at three, P. M.

He is a tall, powerful man, good conformation, sanguine temperament, light complexion, and red hair. Has a wound three-quarters of an inch in length in the left side of the ensiform cartilage, not far from the attachment of the diaphragm. The bleeding had ceased; the wound was dressed with water-dressing. Ordered to have a purgative pill, followed by a dose of castor oil.

Feb. 22. Medicine has not operated; pulse 96, full, and rather inclined to be hard; breathes somewhat quicker than natural; great thirst; tongue covered with a white fur; wound painful, increased by coughing or lying upon his right side; mind much agitated; bowels open at four, P. M.

23. Pulse 104, soft and compressible; complains of pain in the wound on inspiration; less fever.

24. Improving; the wound has begun to suppurate.

26. Attacked in the night with violent pain in the side and dyspnœa; bled to forty ounces with considerable relief.

28. Health much improved; the wound discharges a dark-

colored matter, supposed to be the breaking up of a large coagulum. Made an out-patient on the 7th of March.

March 12. Returned to the hospital to-day, complaining that the wound of the side discharged a great deal on walking; walking and other exertion very much affected his breath, and caused a pain in his side, rather lower than the situation of the opening. Dressed with water-dressing, and a broad bandage to be applied round the chest.

14. Discharge continues very profuse; pressure on the parietes of the chest produces no increase of discharge, though it may be seen to ooze out of the wound during a forced inspiration; his health remains good; tongue clean.

16. Mr. Liston passed a probe to the depth of at least two inches into the wound. A collection of matter is supposed to exist somewhere about the attachment of the diaphragm to the cartilages of the last true ribs. Ordered opening medicine.

18. Mr. Liston made a T-shaped incision, passing through the original wound, and on dissecting back the flaps a small opening was seen passing through the cartilages of the ribs where they were united together, to the left of the ensiform cartilage, through which the matter was seen to proceed. A circular portion of two cartilages, nearly the size of a shilling, was removed with a strong blunt-pointed bistoury, half an inch of the point being broken off, and a large quantity of healthy looking matter gushed out. The bleeding was restrained by lint dipped in cold water, and the wound afterwards dressed with water-dressing.

19. No unfavorable symptoms; the wound discharges freely.

20. Complains of pain on inspiration, and uneasiness around the wound. Ordered three grains of calomel and one of opium every three hours.

22. Pain relieved; slight cough; bowels open.

25. Pain entirely gone, the wound contracting and granulating very nicely; health good; appetite voracious; tongue clean. Allowed to walk out every day.

30. Still improving in every respect.

April 8. Discharged cured.—*Phil. Med. Examiner*, from the *London Lancet*, May 14, 1842.

On the Influence of the Nerves on Muscular Irritability. By M. Longet.—M. Longet found that when a nerve of voluntary motion was cut across, on the fourth day thereafter, the

muscles to which it was distributed could not be excited to contract by irritating the nerve by means of a weak galvanic current. The muscles, however, supplied by this nerve, contract immediately on the application of the slightest stimulus to themselves, even at the end of fifteen days. Even after the lapse of a month, direct stimulation causes them to contract slightly, and may be recognised at the end of seven weeks; no irritation of the nervous twigs, however, excites the slightest motion after the fourth day. From the seventh week, the muscular fibre, already much blanched, seems to undergo a complete degeneration, and soon ceases to contract in the slightest, even with the most powerful stimulants. It is only then in consequence of a lesion of its nutrition that muscular fibre, in losing its organic characters, loses also its essential characteristic, irritability, which, however, as has been seen, remains a long time after all nervous influence has been suppressed.—*Edinburgh Med. and Surg. Journal*, from *Comptes Rendus des Seances de l'Academie des Sciences*, July, 1841.

New Mode of Treating Hydrocele. By J. Pancoast, Prof. of Anatomy, Jefferson Medical College, Philada.—Hydrocele in children, even where the opening has been closed, that led from the tunica vaginalis to the cavity of the abdomen, is a disease of frequent occurrence. In early infancy strong discutient lotions will usually suffice for its cure. But after the second year, some more efficient means are required to produce this result. Mere evacuation of the serum with a common lancet or trochar, or a number of punctures made into the sac with a large needle, so that the fluid may escape into the cellular tissue of the scrotum, and be subsequently removed by the absorbents, are the modes of cure commonly relied on. But I have found them so uncertain in their result, success in many cases being attained only by a repetition of the process, that I have latterly adopted the following plan of treatment, which in three cases that I have tried it in has proved perfectly successful.

I puncture the swelling, in front and below its middle, with a common thumb lancet. When the serum is discharged, a little pressure causes the serous or vaginal tunic to protrude in the form of a small cyst. This I lay hold of with a pair of forceps, and draw it out, as far as it will admit. I then divide the lower half of the cyst next the skin with a pair of scissors, and traction again being made upon the pedicle, still more of

the tunic may be drawn out from the upper portion of the scrotum, which is nipped partly off and treated in like manner as before. I repeat this process while any portion of the vaginal tunic can be made to protrude readily at the opening, so as to be laid hold of with the forceps. I then surround the side of the scrotum and the testicle involved with strips of adhesive plaster, after the manner of Fricke of Hamburg for the cure of hernia humoralis. By this means, the cellular tissue of the scrotum (the tunica vaginalis reflexa having been removed, to a considerable extent, with the forceps and scissors) is brought directly into contact with, and ultimately becomes adherent to that portion of the vaginal tunic which is closely attached to the fibrous coat of the testicle.

The child is allowed to run about as usual, and in a few days is perfectly well. Excepting as regards the puncture of the skin, the operation is entirely devoid of pain.

This plan of cure will, I think, be generally found applicable in children. It is certainly more speedy and certain in its results than any measure short of injection of the sac, which is not usually practised in children.

It would also, I think, be found successful in the recent hydrocele of adults, before the tunica vaginalis reflexa has become so coriaceous, or been so thickened by disease, as to prevent its being drawn out in the form of a cyst through a narrow opening. In one instance, where the puncture or palliative process had been several times tried without success, and in which I feared I might find a thickened membrane, I made the puncture through the skin with a curved bistoury, and pushing it on to the top of the sac, divided with the point of the instrument as I withdrew it, the anterior wall of the tunic, laying open the sub-cutaneous cellular tissue of the scrotum, but not cutting the skin. Subsequently, no difficulty was encountered in drawing out the tunic and removing it with the scissors.

April 20, 1842.

Since writing the above, I have operated upon another case in the same manner, in which the passage leading to the cavity of the abdomen had remained open. The cure in this instance has been slow and gradual, occupying about a month, and without the aid of a truss. The fluid re-accumulated in the tumor during the first week, but it was gradually absorbed, adhesion beginning below and proceeding upwards, until a radical cure was effected.—*Am. Med. Intelligencer.*

June 8, 1842.

First Introduction of Syphilis into Scotland.—At the Medico-Chirurgical Society of Edinburgh, Professor Simpson read a communication relative to the history of the first introduction and diffusion of syphilis in Scotland at the end of the fifteenth century. He cited official edicts of the Aberdeen Magistrates, and of the king's privy council respecting the new 'infirmitez,' issued during the currency of 1497, with various casual notices of the distemper from the contemporaneous writings of Dunbar, Lyndsay, Inglis, &c. In these early times the disease passed in Scotland under the various names of grand gore, gorre, pockis, spanie pockis, French infirmitez, &c. Dr. S. alluded to an entry in 1502–3 in the privy purse expense book of Elizabeth, Queen of Henry VII., as containing the earliest notice of the disease in England, that he had been able to meet with. The first Scottish edicts were made with a view of arresting the progress of the disease, and ordained the application of a hot key of iron to the cheek, as the punishment of those who infringed their regulations. The Aberdeen edict was dated only four years and a month after the return of Columbus from his first voyage to Hispaniola. This edict was remarkable as being founded on the idea that the new malady disseminated itself by impure sexual intercourse—while, as is well known, it was not for several years afterwards, that this opinion of its mode of propagation, was adopted by the medical men and authors of these times. The Edinburgh regulations pre-supposed the disease to be communicable even by the persons that took the cure upon them, although these persons were themselves uninfected. Lastly, Dr. S. proceeded to show from the old data which he had adduced, that syphilis was new to this part of Europe, at the date of the edicts, that it was hence different on the one hand from gonorrhœa, and on the other from the leprosy of the middle ages, both of which affections were well known in this country before the era in question.—*N. Y. Lancet,* from *Lond. and Edinb. Monthly Jour. Med. Sci.*.

Clinical Lecture on Delirium Tremens. By George Budd, M. D., F. R. S,, Prof. of Medicine. Delivered at King's College Hospital, Nov. 20, 1841.—The case of Embleton is a fair specimen of the delirium brought on by excessive drinking. During the years I was physician to the Dreadnought, I had constantly cases of this kind under my care. The frequency of delirium tremens in sailors is very readily explained. When a ship returns from a long voyage, the crew are paid

off. A man receives at once £40, £50, or £60, and is immediately laid hold of by the most despicable of human beings —the crimps, as they are called, or the agents of the keepers of the filthy lodging-houses in the neighborhood of Wapping. He is taken to one of these houses, and kept in a state of constant intoxication for a fortnight or three weeks. By this time his money is generally spent. His clothes are then pawned, and this supplies him with drink a day or two longer. When this last resource fails, when the system of robbery can be carried on no longer, the supply of drink is stopped, and the sailor is turned adrift to shift for himself. In a few days he is brought to the hospital in a state of delirium tremens.

I have known sailors, who had received as much as £70, reduced to complete destitution in the course of two or three weeks. During the time they are kept in a state of intoxication they eat nothing, often not so much as a morsel of bread, for days; they live, as they express it, on the liquor.

When they are brought to the hospital, the condition of all these men is very much alike. The face is flushed, the eyes are suffused, and there is often a peculiar wildness in their air and manner, even when there is no actual delirium. The tongue is pale, moist, and indented; and the hands as well as the tongue are very tremulous. Appetite is quite gone, and they have generally repugnance for food of every kind. In spite of this nervous agitation the pulse is steady and tranquil, and very seldom increased in quickness.

But what is characteristic of the disease, as characteristic as the trembling, is the loss of sleep. They all tell you that they have had no sleep for nights; and that the moment drowsiness comes over them, or even without this, at the approach of darkness only, they are haunted by visions of the most frightful kind; they see serpents about the bed, and are pursued by devils of every size and shape. The constancy of these horrid apparitions has furnished a name to the disease. The term, in fact, by which it is known to sailors, and a very appropriate one it is, is the 'horrors.'

The name has, no doubt, passed down through many generations of sailors. Smollet uses it in Roderick Random. Smollet, in early days, was a surgeon's mate in a man-of-war, and sketched Roderick from the life. Roderick, in relating his history, says, 'I awaked in the horrors, and found my imagination haunted by such dismal apparitions, that I was ready to despair.' This very properly happens to Roderick a day or two after he set sail. In delirium tremens from drinking, the patient generally gets more violent, and in every respect worse,

a day, or two or three days, after he has left off drinking. Sailors are themselves aware that a glass of grog steadies them when in this condition.

In some cases, the men are more violently delirious: they are continually vociferating, and talking in the wildest and most incoherent manner; they are in a state of great agitation and tremor, and their body is bathed in sweat; yet even in these cases the pulse is generally calm and steady.

The character which the delirium assumes is that of hallucination. They see unreal objects, and these objects are almost always of a frightful kind. In some cases, but these are rare, the illusion extends to the sense of hearing. They hear the objects by which they are haunted, talking, and occasionally calling them by name.

But hallucination is more rare for the sense of hearing than for that of sight: probably because more vivid impressions are received through the eye than through the ear. Ghosts are more frequently seen than heard. Dr. Abercrombie thinks that dreams are chiefly occupied with objects of sight. In his work on the Intellectual Powers, he says, 'I am informed by a friend, who is a keen sportsman, that he often dreams of being on a shooting excursion; that he starts his game and points his gun, but never succeeds in firing it. It sometimes seems to miss fire; but in general there is something wrong in the lock, so that it cannot be moved.'

It was probably the poet's own experience that frequent and deep potations give birth to hallucinations of a frightful kind, that suggested to Burns the scene of the witches in Tam O'-Shanter. Tam, returning late after a drinking bout, passes by All Hallowe'en Church, in which he sees warlocks and witches in a dance, with Auld Nick for their piper. The objects with which the fancy of the poet has filled up the picture are certainly frightful enough:—

'Coffins stood round like open presses,
That shawed the dead in their last dresses;
And by some devilish cantrip slight,
Each in his cauld hand held a light,
By which heroic Tam was able
To note upon the haly table,—
A murderer's banes in gibbet-airns;
Twa span-lang, wee, unchristened bairns;
A thief new cuttit frae a rape
Wi' his last gasp his gab did gape;
* * * * * *
A garter which a babe had strangled;
A knife a father's throat had mangled,
* * * * * *
Wi' mair o' horrible and awfu',
Which e'en to name wad be unlawfu',

This description, though highly colored, is true to nature; but such hallucinations usually arise after prolonged excess, and not, as Burns makes it, after a single sitting.

Another character, very general in these cases, which seems allied to the frightful nature of the hallucinations, is despondency and a tendency to suicide. Even in slight cases I have known men suddenly burst into tears while I have been questioning them.

Well-marked cases of delirium tremens are easily recognised. Their characters are so striking, that, taken with the previous history of the patient, they can scarcely be mistaken; but this is not the case with the slighter degrees of the same affection. These are marked only by lowness of spirits, and inability to sleep; or by sleep which is interrupted by frightful dreams. It is a condition which is familiar to many by the name of the 'blue devils,' which, in the majority of cases certainly, must be ascribed to excess in drinking. Men of jovial habits—who take what is called a cheerful glass, and enjoy company and good cheer—are remarkably subject to the blue devils. They are low and desponding in the morning, and their nights are sleepless, or their sleep is broken by dismal dreams. In the language of sailors, they have the 'horrors.' It is the temperate man only whose spirits are uniformly good.

Embleton told us that the objects by which he was haunted, though continually shifting their shapes, were always of a blue color. I have often heard sailors make the same remark; and I am inclined to think that this is a very general character of such apparitions Müller, speaking of phantasms, says that they are generally divested of color.

I have said that in delirium tremens, the pulse, except after considerable muscular exertion, rarely exceeds its ordinary frequency. In cases in which it does, there is generally inflammation of some internal organ.

The organs most frequently inflamed are the stomach and the lungs.

It is not at all uncommon to find a man affected with the horrors, who, in addition to the ordinary symptoms—the delirium and the trembling—has frequent vomiting, a furred tongue, and a quick pulse. Such symptoms generally indicate inflammation of the stomach, and are of very bad augury; because, when they are present, the patient is unable to take solid nourishing food, which is a very important part of the treatment, and, indeed, frequently indispensable to recovery. In a case of this kind I have found the mucous membrane of the splenic extremity of the stomach reduced to a mere pulp.

The lungs are much less frequently inflamed than the stomach; but I have met with two instances of delirium tremens which proved fatal, apparently from the occurrence of pneumonia.

In none of the cases in which I have made an examination after death—in all, four or five—have I found any evidence of inflammation within the head, other than effusion of serum under the arachnoid. The symptoms do not depend on any inflammatory affection of the brain, but on exhaustion, consequent on over-excitement, and in part, perhaps, on the condition of the blood. We can hardly suppose that the blood should not become altered, and for some time remain so, by the continual admixture of spirits. The spirit undoubtedly exerts some degree of chemical influence.

Sailors, in whom I have chiefly observed delirium tremens, are men whose constitutions are not broken by habitual intemperance, but strong, robust men, most of them in the prime of life, who get drunk only while they are in port. From these circumstances the affection in them is somewhat different from what it is in the habitual drunkard; but the main characters and the indications of treatment are alike in both.

The chief remedy is opium; but it must be given in large doses, so as to procure sleep. In small doses, it does comparatively little good. When a man has been for days in a state of nervous agitation, and has had no rest for several successive nights, our object should be to procure sleep. I have often, as in the case of Embleton, seen a man in a state of raving delirium fall asleep under the influence of a large dose of opium, and, at the end of some hours, awake perfectly rational.

The plan into which I settled for the treatment of delirium tremens, such as I used to meet with in sailors, is to give one drachm of tincture of opium, every hour, for three or four successive hours, if the patient should not fall asleep before; and, as soon as he awakes, to give him a mutton chop, or some solid food.

I have already told you that these men, as long as their drinking bout lasts, eat nothing; and although, while the state of excitement continues, they have great seeming strength, and in some cases can be mastered with difficulty, yet, as soon as it subsides, they are always in a state of debility, and evidently require nourishing food. This is indicated, even while the delirium persists, by the condition of the tongue, which is almost always, in these cases, more or less indented; a condition you will generally find associated with states of debility.

Before they have slept they are unable to take food; their appetite is completely gone; a circumstance which must be attributed not to the state of the stomach merely, but to that of the whole nervous system; for the appetite generally returns when the system has been refreshed by sleep.

I consider this point—to give as soon as possible some solid food—a most essential one in the treatment; and have almost invariably found men get worse in cases where, from some mistake or neglect, it has been omitted.

I have said that opium should be given in large doses; but even these sometimes fail in procuring sleep. It is indeed astonishing how much of it can be taken by persons in this state of nervous excitement without sensible effect. In one instance in which I had left directions that a drachm of laudanum should be given every hour until the patient slept, ʒx. were given in the course of twenty-four hours, without producing the slightest sign of narcotism; and it was only after the last dose that the patient fell asleep.

A similar insensibility to the ordinary effects of opium is witnessed when it is given to relieve very severe pain or spasm. Doses, which in other circumstances would be dangerous, may be given not only with safety, but without apparent effect. But although large doses of opium may be given, and are even required, to relieve severe pain, or to control violent spasm, or to calm the nervous excitement of delirium tremens, you must not suppose thst its employment requires no caution in these cases. The rule generally laid down, and I believe it is a good one, is to discontinue the opium when it has produced contraction of the pupil. Different narcotic substances produce different effects on the pupil. Belladonna dilates it; the first effect of opium is to contract it. Contraction of the pupil shows, therefore, that the system is powerfully affected by the drug, and should caution us against its further employment.

When the opium acts favorably, when it procures a quiet sleep of several hours, the benefit resulting from it is remarkable: the delirium has ceased, the agitation has subsided, and, if the appetite have also returned, the patient is convalescent. But in some cases, opium, in however large doses we give it, fails in producing this effect. It calms the patient in some degree, and brings on a dosing state, which is often interrupted by startings, and from which the patient arises but little refreshed.

In many such cases, where opium had failed in composing the patient, I have seen admirable effects from wine or brandy, especially where the patient had left off drinking for several

days. The delirium and nervous agitation seem to result partly from the sudden withdrawal of the stimulus. I have remarked that the men generally are much worse two or three days after they have left off drinking, and that they are aware that a glass of grog steadies them. But I would only give wine or spirit when opium had failed to procure sleep, and when the delirium had persisted some days after the patient had discontinued drinking.

Another important point in the treatment of delirium tremens is, that the patient should be removed as much as possible from all causes of excitement: his room should be quiet, and few persons should be allowed to visit him.

The last point that I wish to impress on you, is, the great importance of narrowly watching the patient. I have already stated to you that in this disease, even in its lighter shades, the patient is possessed by gloomy ideas, and has often a strong propensity to suicide. He should therefore be narrowly watched, and every instrument by which he can effect self-destruction should be placed out of his reach.

For want of these precautions we had a very tragic scene on board the Dreadnought, in the summer of 1838. Three men, Parrymore, Baily, and Forster, were brought into the hospital the same day in delirium tremens. They were treated in the usual manner, and I believe a straight-waistcoat was placed on each. The day after their admission, Parrymore was very much better, and gave me a clear account of many of the particulars of his case—as, of the voyage he had last made, the time he had been in port, and the money he had spent. All restraint was in consequence left off. The same evening he cut his throat with a clasped knife, which sailors generally carry about them. The surgeon, who was in immediate attendance, found him sitting up in bed, bleeding profusely, and still hacking away at his throat, to all appearance perfectly insensible to pain. The divided vessels were tied, and the hæmorrhage stopped; but he died at the end of a few days, from the secondary effects of the wound.

Baily also, the day after admission, gave me a clear account of many particulars of his case; and I considered him in no danger. The excitement and commotion in the ward consequent on the attempted suicide of Parrymore, again threw him into the most violent delirium, in which he continued for two days. On the fourth day after his admission, I found him very hoarse from vociferations, but tranquil and rational. The straight-waistcoat was taken off, and orders were given that he should be narrowly watched. The same evening,

however, he eluded the vigilance of those about him, and threw himslf out of one of the port-holes into the Thames. A boat was immediately sent after him, and he was soon picked up, swimming lustily between the Dreadnought and the shore. When in the boat, he struggled violently to throw himself again into the river; and such was his muscular power, that it was with the greatest difficulty the two men in the boat could master him. They succeeded, however, in doing this, but Baily, apparently exhausted by his struggles, almost immediately expired. He was lifted into the Dreadnought a corpse.

We could not examine the body before a coroner's inquest had been held; and as this was not done for some days, and the weather was very hot (it was the middle of July), the body was too far advanced in decomposition to enable us to form a satisfactory opinion of the cause of his sudden death. I believe, however, that he died of pure exhaustion—the consequence of the powerful efforts he had just been making. He was one of the most muscular men I ever saw, and had no organic disease.

These cases, as you might expect, have made me very watchful over patients in delirium tremens. They should not be left a moment unguarded; and in hospitals where they cannot have a person in constant watch over them, I would always put on a straight-waistcoat, whenever I could do so without using violence or increasing the agitation of the patient. I make this exception, from a sense of the ill effects of excitement, and of the exhausting influence of great muscular exertion.

But delirium tremens, or at least a condition very similar to that brought on by excessive drinking, may be caused by any great shock to the nervous system, especially by surgical operations, or severe injuries.

When induced by this last cause, it is called traumatic delirium. But its characters are precisely like those I have before described:—nervous agitation, trembling, inability to sleep, and hallucinations, generally of a dismal kind.

The insensibility to pain (which I have before noticed in the case of Parrymore) in these cases is truly remarkable. Dupuytren, who has left a beautiful description of nervous delirium, says, that he has known patients in this state, with a comminuted fracture of the leg, slip off the bandages and walk about, using the broken limb without evincing the slightest pain; and others who had been operated on for hernia, introduce their finger into the wound, and amuse themselves

with twisting about the gut, as if they were doing it on the dead body.

He also particularly remarks, as all writers on the subject do, the state of the pulse. He says, 'the most remarkable symptom, in the midst of this disturbance of the intellect, is the calm of the circulation, and the absence of every febrile symptom. You see a patient furious; his face covered with sweat; his eyes glistening; his cries heard at a distance: you believe him affected with the most intense arachnitis. Approach him: his pulse is calm and regular; and the state of his skin removes every suspicion of inflammation.'

But, in other circumstances, you will occasionally meet with a condition of the nervous system very similar to delirium tremens.

In the 19th volume of the Med. Chir. Trans. is a paper by Dr. Seymour, containing three cases of mania, successfully treated by morphia. They all occurred in women.

In the first case the woman became affected, after suffering severe distress, in consequence of the death of a relation who died in her presence. She was occupied with gloomy ideas, and a belief of having committed indescribable crimes. Her nights were sleepless, and there was continual watchfulness, and restlessness of the body. Her pulse was not weak, and her bodily health was unusually robust. She recovered in six weeks, taking a grain of acetate of morphia every night.

In the second case the history is obscure. The woman had been bled and blistered for what was conceived to be inflammation of the lungs. She was extremely violent; fancied that her children were murdered, and their spirits returned to torment them. Her sleep was broken. There was no fever. She recovered under the same treatment.

In the third case there was great previous affliction, and the mania came on after childbirth. The symptoms were nearly the same, and she recovered under the same treatment.

It is impossible not to perceive the analogy between these cases and those of delirium tremens. There is the same absence of fever, the same restlessness, the same want of sleep, and the occurrence of visions and dreams of the same character, in both.

All the instances I have mentioned agree in the absence of fever, and in the regularity of the functions of the body in the midst of the general nervous disorder; in the presence of gloomy ideas and frightful dreams.

It seems, then, probable that opium will be beneficial when ever there is a combination of these conditions. Dr. Seymou-

says,—'Morphia appears to be of more use in what is termed melancholia, where the mind is tortured by imaginary want, ruin, or crime, than where the derangement partakes of more brilliant ideas; as of superhuman knowledge, dexterity, or wisdom; or than in those cases where one single idea occupies and absorbs the mind.'

I have seen one or two instances in which a state very like that in the cases related by Dr. Seymour, resulted from excessive study.

One was in a man with whom I was well acquainted at Cambridge. He became insane after an attack of scarlatina in the summer of 1828. He had been reading excessively hard, and was much exhausted, when he was attacked with scarlatina on his way to the Lakes, to spend the summer vacation. He was laid up with scarlatina a fortnight; and soon after was placed in an asylum, where he remained two years. At the end of this time he was released, resumed his reading at Cambridge, and took a wrangler's degree in the year after that in which he would have taken his degree in the ordinary course.

The character of his insanity was like that of persons in delirium tremens. He fancied that he was constantly surrounded by devils; and even now, ten years since leaving the asylum, he is convinced of the reality of these apparitions.

My attention has lately been called to another case of the same kind. A man, like the former, became insane after hard reading; and his insanity was of the same character. He was placed in an asylum, where he continued some years. Since his release he has turned author, and the books he has written display considerable talent.

I have little doubt that such cases are allied to delirium tremens—that the insanity is the cause of nervous exhaustion—and that it is best treated by opium, generous living, and quiet; means so efficacious in the various forms of delirium tremens. It would certainly afford a medical man a high gratification to cure a case of this kind—to restore an educated and gifted man to his family and himself; but there is very little satisfaction in curing a man of the delirium that results from drinking, because he is almost sure to get into the same state again. The only chance for him is to become a tee-totaler.

Experience of the 'horrors' has certainly very little influence in deterring sailors from a repetition of the excesses by which they are brought on. Embleton told us that he had had the 'horrors' twice before; and men have often confessed to me that they have had them as often as seven or eight times.

I am afraid, therefore, that Burns' parting advice, in Tam O'-Shanter, has been lost on the lovers of tippeny:—

> 'Whae'er this tale o' truth shall read,
> Ilk man and mother's son tak heed;
> When'er to drink you are inclined,
> Or cutty sarks rin in your mind,
> Think, ye may buy the joys owre dear;
> Remember Tam O'Shanter's mare.'

Experiments on Kiesteine, with observations on its application to the diagnosis of pregnancy. By Elisha K. Kane, M. D. The following extracts are from a valuable paper under the above title, in the last number of the *American Journal of the Medical Sciences.* The paper was an inaugural dissertation for the degree of Doctor of Medicine, and is published in accordance with the recommendation of the Medical Faculty of the University of Pennsylvania.

"My mode of conducting the experiments was this. The recent urine was placed in open glass cylinders, of diameters varying from an inch and a half to that of a common tumbler, and protected from the dust by paper covers. Those were arranged in a dry, well ventilated room, where the temperature was uniform and moderate, and were exposed in groups to the equal action of air and light. I examined them frequently during the day; but as the changes were not rapid, I determined after a little while to note only one set of observations in the twenty-four hours. My notes were made upon the spot. If from any cause, an individual observation or a series was unsatisfactory, or inconclusive; or if it led to a different result from others, I repeated it at once with increased care; and I was always careful to observe the constitution, habits and circumstances generally of each patient.

The examination of the first group of cases satisfied me, that the urine during pregnancy assumes appearances different from those witnessed under other circumstances, and which I was therefore disposed to regard as characteristic of that state. Subsequent inquiries confirmed me in the general accuracy of this opinion, but compelled me at the same time to admit its liability to exception.

The more obvious of these appearances regard the superficial formation described by Dr. Bird, and recently investigated by Drs. McPheeters and Perry; but there are others which point to a series of intestine changes somewhat more obscure,

though scarcely less interesting. My observations applied to both; and I regret that the limited time at my command during the studies preliminary to graduation, obliges me to select a single class as the subject of this dissertation. I take, however, that indication which seems to be best fitted for practical usefulness in diagnosis, the *pellicular change*, and which I suppose to be most properly called the Kiesteine.

The urine, submitted to observation in the way I have described, presents but little change during the first thirty-six hours. The mucous flocculi, if they exist, gradually subside during this period, forming a whitish cloudlike deposite at the bottom and sometimes on the sides of the glass; while more or less alteration occurs in the color and tranparency of the fluid.

The surface remains for a short time entirely unchanged; but in most cases, a greater or less number of shining acicular specks, apparently crystalline, begins to be seen within the first eighteen or twenty-four hours. These are generally scattered over the surface without regularity; but in some rare cases, they are so disposed as to form a translucent film of uniform thickness, which afterwards assumes the more defined characters of the pellicle. How far these crystals are essentially connected with the formation of the pellicle, I am not prepared to say. In many cases, I have not succeeded in detecting their presence, even by a microscope; and, indeed, I have failed to discover any *unvarying* indications whatever of the approaching development of the kiesteine.

The cloudlike appearance, which is alluded to by Nauche and Eguisier, although possessed of much interest, I have not found to be a uniform premonitor of the forming pellicle; I have supposed it to be nothing more than the Enœorema of the older writers, depending upon the imperfect aggregation of mucous flocculi; for I have seen it repeatedly when there was no pregnancy to account for it, and it was uniformly absent where the fluid presented perfect transparency.

The time at which the pellicle begins to form varies considerably. I have seen it well marked at the end of thirty-six hours, and have known it to make its first appearance as late as the eighth day. At first, it is hardly discernible. It is generally seen forming at the centre or on the sides of the glass, presenting a delicate milky or bluish white aspect. It is however in some cases uniformly disposed over the surface from the commencement, and assumes the appearance of a nearly transparent film, which gradually becomes more distinct. But it has not always the continuous strongly marked

character, which some have ascribed to it. I have seen it begin in striated irregular lines, somewhat resembling a spider's web, in rings, circles, trapeziums, and irregular figures of almost every shape, which gradually became obscured by the full development of the pellicle.

When it has attained this stage, which occurs generally about the fifth day, it presents a continuous scum of an opaline white or creamy appearance, with a slight tinge of yellow, which gradually becomes deeper and more decided. The uniformity of this color, however, is generally broken by granulated spots of a clearer white, giving it a dotted or roughened aspect. The crystals of the forming stage now appear like shining points, and I have sometimes found numerous small brownish specks, sprinkled over the surface, not unlike the gratings of nutmeg. It is at this period, that the pellicle may be compared "to the fatty scum of cooled broth."

In this state it continues for some time, preserving all its characters unbroken. The glass, where the surface meets it, is discolored by a white opaline ring; and a series of such rings, varying in extent from a line to the fourth of an inch, marks the descent of the surface during the progress of evaporation.

The cheesy odor, mentioned by Dr. Bird as a valuable aid in diagnosis, and as "by no means unfrequent in those specimens in which the pellicle is very thick," I have found in but seven cases. Many pellicles of great thickness were entirely without it; and in two of those presenting it, the pellicle was thin and not very well developed. Drs. McPheeters and Perry were unable to detect it in either of the twenty-seven cases examined by them, and I have found it unequivocally developed in at least three cases in which pregnancy did not exist.

The pellicle, if left undisturbed for some days, breaks into cracks, commencing generally from the central portions, but not always extending to the edge of the glass. These are again crossed by other fissures, and the pellicle is more or less broken up. In the mean time, the flakes, which have been forming from the commencement of disintegration, have their edges depressed into the fluid, while at the same time the general thickness of the pellicle is much diminished; and this depression or dip gradually increasing, the depending particle s detached, and sinks slowly to the bottom. Its complete disintegration, however, is but seldom seen; being anticipated by the decomposition of the fluid. The deposite is of course considerably increased by the fallen portions of the pellicle, and is found irregularly disposed over the bottom of the ves-

sel; but as I have remarked, most abundant on the side farthest from the light.

I cannot agree with those who consider this deposite as presenting well marked distinctive characters to the eye, and I certainly have not found it uniformly coincident with the approach of the pellicle. It has indeed in many cases been absent at that period; and in others, until augmented by the detached pellicle, I have been unable to distinguish it from the very many deposites found in other urine. How far a chemical investigation may give it value, I am not prepared to say: although its liability to be confounded with other sediments makes it practically unavailable as a test, it offers a fine field for microscopic and chemical research.

This description of the appearances and changes of the pellicle, though more detailed than those of Nauche and his followers, still applies only to the better defined examples. In a considerable proportion of cases, some of the phases mentioned are not to be found together; and I have not been able by the most careful observation to discover the causes of variance. This I allude to here, because the absolute and unqualified language, which I have met elsewhere upon the subject, seems to me calculated to mislead the unpractised inquirer.

It must not however be inferred that the presence of kiesteine is determinable only by vague and undefined characteristics. On the contrary, the tables which are appended to this dissertation, will show that they are as well defined as most pathological phenomena, though like them they sometimes require for their discrimination a practised comparative scrutiny.

The table marked A exhibits a condensed record of my observations, more or less frequently repeated, on the urine of eighty-five pregnant females. Of these, as will be seen, sixty-eight gave a well marked pellicle of the sort called kiesteinic, eleven gave the pellicle under a modified form, but with appearances which enabled me to recognize it clearly, and six gave no pellicle whatever. Of these last, one was laboring under mammary abscess and convalescing from typhoid fever, and one was in a condition of extreme anæmia from repeated uterine hemorrhages; but the others, unless they succeeded in practising reiterated deceptions on me, which I can hardly believe, must be regarded as absolute exceptions."

"The urine of unimpregnated females in a state of health rarely undergoes any change which in this respect can be misapprehended. I have examined twenty-eight cases in a perfectly healthy condition, and have sometimes known pel-

licles form on the urine, as well as on that of males; but the distinctive character of the kiesteine was wanting in every case.

In certain pathological conditions, however, discrimination is somewhat more difficult. The pellicle that is not unfrequently seen on the urine in the last stages of phthisis, in arthritic diseases, and in cases of metastatic abscess, vesical catarrh, and uterine tumours, has points of resemblance to the kiesteinic which might readily mislead the unpractised."

"The particulars in which the kiesteine differs from other pellicles regard the manner of its formation and departure, even more than its appearance when developed. As I have already mentioned, it generally begins to show itself within a day, or at furthest within two days after the discharge of the fluid, and advances gradually to its complete development. The other pellicles, on the contrary, rarely give indications of their approach until the fluid has stood a longer time, or even till decomposition has supervened, and then form with rapidity. I have known them entirely defined within a few hours. The kiesteinic pellicle, when fully formed, has almost always a much greater degree of tenacity than the others: I have often, for purposes of microscopic examination, lifted large flakes entirely out of the urine; and when it was well defined, this was easily done: with the others it was never practicable. It seems also to be independent of putrefaction; it is not obscured for some time by the disorganization of the liquid on which it rests; and the characteristics which I have already described as accompanying its appearance are very seldom simulated.

The appearances which I have observed seem to point directly to the conclusion, that the formation of the kiesteine is unconnected with the presence of extraneous pus or flocculent mucus. I was aware that these and other animal matters might under certain modifications give rise to a scum upon the surface, the "cremor urinæ" of the older writers. This has been noticed by M. Becquerel, as especially observable in leucorrhœa; and I have observed it very frequently, not only in that disease, but in cystitis, gonorrhœa, vaginal and uterine hemorrhages, and immediately after delivery when the lochial discharge was mingled with the urine."

"In the present state of physiological chemistry, but little can be determined with regard to the nature of the kiesteine, and its very doubtful claims to be considered as a new principle. The absence of coagulation by appropriate agents indicates in a measure that neither caseum nor albumen exists in

perceptible quantities; while the acid reaction up to the moment of disintegration seems opposed to the idea of its being a mere attendant upon increased quantities of pus or mucus."

"The pellicle, taken immediately from the urine on a glass plate, carefully introduced, when examined with a magnifying power of one hundred diameters, exhibited, while yet moist, a well defined series of flakes of a somewhat darkish yellow, made up apparently of minute granules. This appearance, which I at first thought to consist of minute globules of mucus or pus, was at once recognised by Dr. Goddard, as closely resembling, if not identical with, the granules of the colostrum. Having with some difficulty procured a supply of this fluid, a comparison of the two exhibited still more clearly this interesting resemblance. The granules of the kiestine were however more flattened than those of the colostrum, a change they might readily have undergone during their passage through the kidneys; but the general aspect of the two was such as to give strong evidence of their identity.*

Connected with these appearances, and sometimes obscuring them, the kiestine presented under the microscope an irregularly disposed amorphous matter, sometimes arranged in groups of granules that resembled the urate of ammonia,† and sometimes of badly marked globules, allied to those of pus or mucus, and accompanied by laminæ resembling epithelia.‡

Throughout the field of the instrument was seen, in varying numbers and distribution, a series of rectangular rhomboidal prisms, more or less distinctly marked, and strongly refracting light. The triangular prisms were also occasionally distinguished, but not in the "myriads" seen by Dr. Bird; and sometimes other crystalline forms were observed in addition to these. They all belong most probably to some of the earthy phosphates. Not only were the triangular prisms recognised to be those described by Dr. Bird, as belonging to the ammoniacal phosphate of magnesia, but by comparing them with the microscopic plates of Rayer and Vigla, and Mandl, (Etude Microscopique sur l'Uurine,) many others were discovered, coinciding with the varied forms of this prominent salt."

"In the pellicle not kiesteinic, the peculiar granular arrange-

* This resemblance was very striking upon comparing it with the plates of M. Mandl.

† See Mandl, Raspail, and Rayer.

‡ See plates of Rayer and Vigla, Encyclgraphie Medicale, Vol. VI.

ment noticed as so strongly resembling the colostrum, was in no case present. Amorphous darkish masses, accompanied by various crystalline forms, were seen only.

"Where much discoloration existed from sanguineous or bilial intermixture, a strong resemblance was observed to the globule of the blood."

"I cannot regard the kiestine as an *unerring* test of pregnancy. I have already shown that it is present under other conditions of the system; and even where pregnancy exists, I am not satisfied that this indication is not always observable.

I am convinced too, that the kiestine is not always distinguishable from other pellicles which appear on the surface of the urine. At least, I am bound to say, that, in the absence of other indications, I should sometimes have found myself unable to distinguish between them. Not that they are generally liable to be confounded, but between the imperfectly developed pellicle of the one character, and the best simulation of it which is sometimes presented by others, the distinction is too slight to be satisfactory or satisfying.

But with the qualifications which these remarks imply, I have no doubt that the pellicle which has been denominated kiestine *is among the best*, if indeed it be not *the most certain*, of the earlier indications of pregnancy. I resorted to it habitually in my diagnosis in the obstetric wards of our hospital, and with constantly increasing confidence."

"The result of my observations may be summed up in the following general conclusions:

1. That the kiestine is not peculiar to pregnancy, but may occur wherever the luteal elements are secreted without a free discharge at the uterus.

2. That though sometimes obscurely developed and occasionally simulated by other pellicles, it is generally distinguishable from all others.

3. That where pregnancy is possible, the exhibition of a clearly defined kiestine pellicle, is one of the most equivocal proofs of that condition: and

4. That when the pellicle is not found in the more advanced stages of supposed pregnancy, the probabilities, if the female be otherwise healthy, are as 20 to 1 (81 to 4) that the progress is incorrect."

"Since this dissertation was submitted to the Medical Faculty of the University of Pennsylvania, two papers on this subject have appeared: one by Mr. Lehmig, in the *London Medical Gazette* of Dec. 24, 1841, and an elaborate paper by Dr. Stark, in the *Edinburgh Medical and Surgical Journal* for January of the present year.

The facts observed by Mr. Letheby accord generally with my own. He found "unquestionable evidence of kiesteine in forty-eight out of fifty cases between the second and ninth month of utero-gestation," and was unable, like myself, to "account for its absence in the two exceptions." In seventeen non-pregnant women he found no indication of its presence; but detected it in the urine of ten suckling women, immediately after delivery, and onwards to periods between the second and sixth months, when it disappeared. The few microscopic results which he gives coincide also with my own.

Dr. Stark's paper is devoted to the signs of pregnancy, and among the rest to the state of the urine. He refers succinctly to his own observations relative to the kiesteinic pellicle, which he supposes to be derived from the suspended sediment; and he asserts that there exists a relative proportion between these and the earthy salts which enter into the composition of the urine. His paper derives its principal interest from his researches into the character of the sediment.

In the natural sediment of the urine of pregnancy, he was unable to detect the presence either of albumen or caseum by acids, alkalis, or alcohol, with the aid of heat; but when he added a certain quantity of milk to the urine, both these principles were discovered readily by the aid of acids, the other tests producing no effect; when milk was added in smaller quantities, however, it was undiscoverable by any reagent. Being unable to refer the sediment to any of the known deposites of the urine, though his investigations had immediate reference to its distinctive characters, he employed ether to effect a separation of the animalized matter which he thought it might contain, and he supposes that he succeeded by this means in detecting a substance entirely different from any heretofore known.

Failing to determine its constitution by chemical agents, he resorted to the microscope. He here found that this sedimentitious matter, whether examined while yet held in solution by the recent urine, or when it had assumed the form of a deposit, or when it had been disengaged by ether, was composed of distinct transparent or "pellucid" globules, which when in their sedimentary condition bore a striking resemblance to the caseum globule of recent milk, but which when pellucid bore an equally strong resemblance to the serous or albuminous globule.

Dr. Stark now reduced the question to very narrow bounds, by inquiring as to the identity of this sedimentary matter with albumen, caseum, fibrine, and gelatine. He thinks that its

minute structure and chemical properties sufficiently distinguish it from the two first;—from albumen, because it dissolves, instead of coagulating, upon the application of heat; from caseum, because it is soluble in nitric and sulphuric acids, which exert on this principle a very contrary action. From fibrine, it has necessarily a still greater difference. There remains *gelatine;* and compared with this, the distinction was less striking. The globules of both under the microscope, were similarly formed, both were soluble in the acids and alkalies, and by the aid of heat. The only distinctive particular seems to have been the action of tannin, which, as is well known, precipitates gelatine from its solution in water. Some of the natural sediments, dissolved in boiling water, and cooled to a blood heat, were treated with a decoction of galls: a flocculent precipitate was at once produced; but instead of gelatinizing upon cooling—*it was deposited*; and instead of becoming more solid and more easily separable, upon reboiling, it again underwent solution.

Upon these grounds he attains the conclusion, which I give without comment, "that this substance is a matter *sui generis*, an elementary substance or principle, forming in some measure a connecting link between the albuminous and gelatinous elementary principles.' This substance he proposes to designate by the name of '*Gravidine.*'

It is unnecessary to say, that this discovery of a new organic principle, if confirmed by future investigation, will be a matter of great interest. I must confess, however, that the distinctive characteristics of the new substance do not seem to me very decidedly marked in the results announced by Dr. Stark; and such is the complex, and often deceptive nature of the investigations of physiological chemistry, that we have a right to wait for renewed experiments before admitting too implicitly the certainty of those he has described.

Dr. Stark considers that his experiments entirely subvert an opinion which has met with some favor regarding the theory of these appearances. Ever since the publication of Nauche's paper, the supposed presence of caseum in the urine of pregnancy has countenanced the idea entertained by Bird and others, already referred to, that the elements of the milk (not, as Dr. Stark infers, the milk itself,) might probably exist in the urine:—as, however, the matter is neither milk nor caseum, a theory based upon their presence must necessarily fall. The conclusion may be a correct one so far as the chemical analysis is concerned; and yet the connection between the kiestine pellicle and the mammary secretion may be adequately

proved by other evidence. If even the Gravidine be regarded as a new organic principle, its properties are not so peculiar, nor its analogies with caseum so remote, as necessarily to imply the operation of different causes in the formation of the two.

I have already mentioned my conviction, founded on personal observations, that the unmodified caseum is not found in the urine; but the presence of the colostral appearances under the microscope, and the numerous phenomena which I have described as attending the presence of the Kiesteine leave me no room to doubt its *intimate connection with the condition of lactation.*"

Dentition of Children at the Breast. By Professor Trousseau.—Suspecting that the generally received ideas on the dentition of children were not correct, M. Trousseau, from statistical observations not very numerous, has come to conclusions slightly different from the opinions generally believed.

Period at which the first tooth appears. This has been determined in 25 children; 13 boys, and 12 girls. It appeared as follows:

In 2 boys at 3 months.	In 1 girl at 3 months.
2 at 4	2 at 3
2 at 5	1 at 4
2 at 6	1 at 5
2 at 10	3 at 6
2 at 11	1 at 7
2 at 14	3 at 9
	1 at 14
Extremes, 3 months, 14 months.	2 months, 14 months.
Average, 7 months.	6 months.

The average time, then, at which the first tooth appears is, from these results, six months and a half; while the general belief at present is, that at eight months it makes its appearance. The common opinion that little girls are more precocious than boys, is also corroborated, as we find a difference of a month between the appearance of the first tooth in boys and in girls.

The first tooth is in general one of the middle inferior incisors. Of twenty-eight children this was the case in twenty-five. In two, a middle superior incisor appeared first; and in a little girl, the first molars preceded all the others.

Period at which the second tooth appears. The twenty-five children in whom a middle inferior incisor first showed, had for their second tooth, the other middle inferior incisor. The common opinion is, that this second tooth appears almost at the same time as the first; and it is quite correct. In twenty-five children, the time that elapsed between the first and second tooth, was, 1 day in 4 children; 2 days in 2 children; 3 days in 1 child; 8 days in 3 children; 15 days in 7 children; 30 days in one child; 90 days in 1 child. In six the time could not be determined by the mother.

The middle superior incisors appear next after the two middle inferior. In eighteen children, fifteen had these teeth after the middle inferior incisors. The time that elapses generally between the cutting of the inferior and superior incisors, is more variable than is generally supposed. In thirteen children in whom M. Trousseau was able to verify this; there was a lapse of 8 days in 2 children: 1 month in 3 children; 2 months in 3 children; 3 months in 1 child; 4 months in 1 child; 5 months in 2 children; 10 months in 1 child.

In general, then, there is a considerable interval between the appearance of the second first and the second second teeth. It may be established as a general rule, almost without any exception, that after the appearance of these four teeth, the two lateral superior incisors next cut the gum, and then the two inferior ones, so that the child has only two teeth in the lower jaw, while it has four in the upper. It is curious, M. Trousseau says, that this fact, so well known to mothers who have brought up several children, should be unknown to those who have written specially on the subject. Whilst a considerable period takes place between the first appearance of the inferior and superior incisors, the four superior incisors follow each other at a short interval. After these six teeth have come through, some time elapses before the next, which are generally believed to be the two lateral inferior incisors, appear. M. Trousseau, however, says, that as a general rule, one or several molars, and sometimes even the canine, are protruded before them. After the cutting of the four molars, and the two inferior lateral incisors, another considerable interval of time elapses, when the four canine successively appear; and then after another period, at the age of from twenty-four to thirty months, rarely sooner, the second set of molars come out.

In the second part of his work, M. Trousseau treats of the diseases incidental to teething, and especially of the diarrhœa accompanying it, and he combats the opinion so prevalent,

that nothing ought to be done for it. He shows that it is not only not useful, but decidedly injurious to the little patients—that it weakens them, and lays the foundation of marasmus, which if not checked in time, soon goes on to a fatal termination.—*Lond. and Edin. Monthly Jour. Med. Sci.*, May, 1842, from *Gaz. Méd de Paris*, 4th Feb. 1842.

Expulsion of a mass of hair from the Uterus.—By Henry R. Frost, M. D., Charleston, South Carolina.—To give full interest to the above extraordinary occurrence, it is necessary to detail the circumstances connected with a case of tedious and instrumental labor.

A colored girl aged 25 years, pregnant with her first child, was taken with the pains of labor, about 6 o'clock on the evening of the 9th of September, 1838. The habit of the patient was strong, and her health good. There was no occurrence during the first twenty-four hours to excite uneasiness, but the delay which occasioned the accoucheuse to suppose that there was something wrong.

On the evening of the 10th, I was called to see her. Upon examination, I discovered nothing at fault, but the slowness of dilatation in the os uteri. There appeared to be some thickening of its orifice. The liquor amnii was passing off slowly, the hairy scalp could be felt protruding, and the vertex resting upon the brim of the pelvis. To favor relaxation, a dose of castor oil had been administered in the morning, and a small quantity of blood taken from the arm. Expecting that the pains would be renewed, and finding nothing requiring my assistance, left the patient in charge of the midwife.

Sept. 11th.—This morning at 9 o'clock, find upon examination, that the vertex had made little progress; the os uteri being a little dilated, and thinner; repeated the venesection.

1 o'clock.—But little progress made, and as the pains were exhausting without producing much effect, recommended that they be allayed as much as possible; directed acetate of morphia in doses of one-sixth of a grain every half hour until easier.

7 o'clock.—The medicine had been taken, and some relief experienced.

12*th.*—The pains were removed during the night, but with little benefit. Upon examination, but little change was perceptible; the head had descended a little lower, and the protruding scalp filled up the os uteri. Apprehending that there

might be some obstruction in the passage of the head, and that difficulty would occur in the course of the labor, requested assistance.

The late Professor Wagner was called, and upon examination of the patient, concurred in the opinion I had formed of the presentation; recommended patience and perseverance. The powers of the patient were good, and as the pulse was more active than proper, further venesection was recommended.

The condition of the patient was little altered during the day and night.

13*th*.—Still trusting to nature, and nothing done to expedite delivery. At 8 o'clock P. M., the forceps were applied, but with no advantage. In the course of the night, the ergot in infusion was freely administered, but without any effect.

During the day a discharge of meconium in considerable quantity was noticed, an occurrence in a presentation of the vertex very unusual.

In the progress of the labor, it became necessary to draw off the urine twice a day, but it is somewhat singular, that though the catheter was introduced, a very small quantity of urine was removed, while a tumor which was formed by the distended bladder could not be reduced, though pressure was made upon it.

14*th*.—At 9 o'clock, A. M. the head was perforated and the child extracted. The operation was completed in about half an hour, and was well supported by the patient. The placenta was extracted, and the patient made as comfortable as possible; some soreness and tumefaction was experienced, and at night nearly two quarts of urine were removed.

15*th*, 16*th*, 17*th*.—The patient continues to improve.

Nov. 17*th*.—The progress of the patient was a very slow, but gradual amendment. About this period, she complained of fever, which continued at variable periods during the day, followed by very profuse perspiration. There was considerable discharge from the vagina of a puriform and very offensive fluid, pains about the region of the pubes at times considerable, occasionally tumefaction of the abdomen, the appetite variable. To relieve some of these symptoms, general treatment was adopted, and the use of an astringent injection.

19*th*.—The patient informed me that the vagina was filled with a substance, which prevented the introduction of the syringe, that sitting was very uncomfortable, and the feeling of something which was to be removed.

Upon examination, I discovered a substance of a dark color,

presenting at the os externum. I provided myself with a pair of forceps, and with little trouble extracted a mass, several inches in length (say 5 inches), and an inch and a half in diameter, at its largest part, looking like wet tow, of an irregular and somewhat pear-like shape. Carefully inspecting it, it proved to be a mass of hair, in short pieces, of an inch to two inches in length, very offensive, and saturated with a purulent looking fluid. The symptoms above noticed soon subsided, and the person recovered in a considerable degree. The preparation is now to be seen in the museum of the Medical College of the State of South Carolina.

The preceding occurrence affords a subject for speculation and inquiry.

It could not have been an offcast from the first child, the quantity of hair being too considerable, and of a different texture from what is usually found on a colored child.

It must have existed in the uterus at the time the first child was born, and must have remained in the cavity of this organ, and the vagina, nearly two months after the delivery. This is more remarkable, inasmuch as the placenta was removed after the birth of the child, and the condition of the uterus carefully examined.

When removed, it had no appearance of being enveloped with membranes.

Was it an abortive effort of nature in the production of twins? or was it of that class of morbid growths, which are so frequently formed in the ovaria?—*Am. Jour. Med. Sci.*

Structure of the Small-pox Pustule.—Mr. W. H. Judd has communicated, in the *Lancet* (June 12, 1841), some interesting observations on the structure of the small-pox pustule. Mr. J. has dissected the skin whilst it was under the influence of variola, in its various stages, and has discovered, he states, a very extraordinary septum and band in the small-pox pustule. The eruption, he remarks, commences by little red points on the skin resembling flea-bites. This appearance Mr. J. found "to arise from increased vascularity, caused by zones of minute vessels enlarging and projecting from the surface of the cutis; they secrete a thin serum, which gradually raises a ring of the cuticula externa from the rete mucosum, and so forms a vesicle without breaking up some of the thread-like attachments and ducts in the centre, between the cutis, rete, and cuticle. Hence the cuticular covering of the vesicle is

bound down at this spot by that thread-like band, which causes it to have a peculiar depressed summit. As the disease advances, the efflorescence and inflammation increasing, the serum becomes gradually more opaque, and coagulable lymph is next thrown out, which at once consolidates and forms a thin flat layer, or plate, shaped like a cymbal, but with a small hole left through its centre, from the circumstance of the coagulation taking place around the before mentioned thread-like attachment of the cuticle. About this period the fever and inflammation are increased (called the secondary or suppurative fever); and in this stage pus being secreted instead of lymph, it elevates the lately described cymbal-like plate, and causes it to divide the cavity horizontally into an upper and lower cell. As the disease advances, the progressive distension breaks up the remaining attachment between the cuticle and cutis: the pus in the cell below passes through the hole in the cymbal-like plate, or septum, above, and blends with the serum in the vesicle, changing it into a pustule (called the maturing stage). By this time the lower part of the pustule is completed by an extremely thickened state of the rete mucosum, which forms a raised lip, or cup, constituting its base; so as, in ordinary cases, to shut off and protect the cutis from the contact of pus and ulceration. Hence, in most instances, the pustule may be stripped off with the cuticle and thickened rete, leaving the cutis entire; but the cutis vera has frequently a slight depression, or pit, left from the effect of the ulceration which has penetrated the base of the cup; and occasionally a small papula or two of the cutis is found projecting into its centre, to which the thread-like band of attachment from the cuticle still adheres."

Mr. Judd next describes the decline of the pustule and its consequences. "About the eleventh day (varying a day more or less in different epidemics) the cuticular covering bursts a little to one side of the apex of each pustule, and its contents gradually ooze out. If the affection has run through its stages mildly, the chief part of the fluid evacuated is but opaque lymph, which drying, forms a dark crust; if, on the contrary, the disease has been severe or confluent, an offensive pus is let out, which leads to scabs, sores, and ulcers in various parts. In bad and confluent cases, where inflammation has run high, not only is pus formed in contact with the cutis at the base of each pustule, but there are also thrown off shreds or sloughs from the true skin, causing permanent depressions, or white pits, for life. Temporary red stains are always left upon the skin, for a time after this eruption, and are caused by increased

vascularity, thickening and rising of the rete; but these disappear if no ulceration takes place.

"Having anatomically traced the cause, we are now fully prepared to enter upon the second stage of our inquiry; viz. the prevention of the effect, or pits, and consequent deformity. In the first place, I shall premise the propriety of lessening, on the attack, the attendant fever and heat by all prudent means; as the height to which the edges of the cup, or scar, will rise, and the depth to which ulceration, or slough, will proceed, depend in a great measure upon the degree of inflammation and fever (though some families are certainly also influenced by constitutional peculiarity;*) but, commonly, I cannot advise blood-letting, especially to any extent, on account of the debility that invariably follows this disease; nor can I praise the use of antimonials and medicines that determine to the skin, as by increasing the cutaneous circulation they give rise to a fuller development of the pustules. I believe the safest and best mode of treatment is to give a brisk cathartic of calomel and jalap; to keep the patient on a strictly antiphlogistic diet; to put out the fire and lighten the clothing, so as to keep the sufferer's temperature as near the standard of health by the thermometer as possible, yet not so low as to impede a fair development of the eruption on most parts of the body. Nor must we abstract food, for so many days in succession, as to let the system flag from that cause; for a certain degree of vigor is needful: but all beyond this does harm. Just after maturation the patient will require support; and in some cases, meat, wine, or even bark. I should not have touched upon treatment, had not the size of the pustule, and consequent extent of the pit and deformity that will remain after the eruption, so much depended on it: and also the prevention of cerebral symptoms, sometimes said to occur on interfering, or suppressing, much of the general eruption.

"I shall next remind the reader, that the face, arms, and parts commonly exposed to light and air, always, as we might anticipate, suffer most deformity by small-pox pits; for air and light give vigor to arterial action, darkly tint the leaf, and heightens color in the cheek and pits. Hence small-pox pustules on the tongue, throat, and parts excluded from their agency, are nearly white, and seldom fully developed; and hence the success of Dr. Picton's method of preventing the ulcerative process, pits and stains, by the exclusion of light

* As is more fully described in my treatise on Syphilis, p. 122.

and air from the chamber of the sick. But in doing this the opposite extreme must be avoided; for it is well known that warmth and moisture (for you can scarcely exclude the air without increasing warmth and moisture) increase suppuration, and materially influence the development of all eruptions: even a poultice applied to a vaccine *vesicle*, from its warmth and moisture, will so alter the action of its secretory vessels, as in twenty-four hours to convert it into a *pustule*. Destroying the pores and the transparency of the skin by the application of nitrate of silver, successfully practised by Dr. Serres in this disease, amounts, in my opinion, only to the exclusion of light and gases.

"Mr. Le Grand's plan of smearing the surface with gum-mucilage, and covering with gold beater's skin, also lessens the action of light and excludes air; but it advances a step farther, by exerting another powerful curative agency, viz. an uniform pressure. Which reminds me of a method I have pursued for years, that of curing pustules, vesicles and sloughing ulcers on the face, &c., by a stiff coating of hot yellow wax and oil, after they had resisted all common treatment.

"Mr. George's plan of caking over the surface with calamine, acts not only by excluding light, but by absorbing moisture, and through its astringent qualities causing contraction of the living pustule. I have long since so applied the white oxide of zinc, and found it to act in a similar way upon vesicles, causing them to break, part with their contents, and heal. A coating of pure liquor plumbi acts in almost the same manner; and many eruptions about the face, hands, and neck, that have been obstinate, cease to come out, and speedily heal, if light and air be excluded, and motion restrained by uniform pressure; though effected if only by adhesive plaster and a roller.* Thus we approach by degrees, and in effect, almost to the method recommended by Dr. Olliffe; viz. the application of emplastrum ammoniaci cum hydrargyro, which in itself seems to possess the curative qualities of most of the other methods, with the valuable addition of power to promote absorption of the serum and lymph, when applied early in the disease, or even of the raised edges of the cup-like bases, which give rise to pits, scars and seams in a later stage. If the serum and lymph be thus caused to be absorbed, the vesicles are not completed, and therefore the pustules do not

* I have not noticed the early puncturing of each pustule, which is useful to prevent pits, and but anticipate Nature's plan.

occur. And this power over the disease we may fairly consider the remedy to possess, knowing, as we do, that the absorption of new-formed parts can be so much more easily promoted than that of originally formed structure. Any other specific action from the mercury and ammoniacum appears very unlikely; and as to animalcula causing the pits, Dr. Olliffe may certainly dismiss that idea from his consideration, notwithstanding that animalcules exist in all long-retained secretions in the human body.

After numerous dissections and microscopic examination, I may affirm that this eruption does not commence by enlarged papillæ, but by simple zones of minute vessels projecting from the cutis, throwing out serum, and raising the vesicle: and that although the red depositions protrude beneath the cuticle, so as to be felt on the surface, it is merely from distension; and enlarged papillæ are not to be found until after the maturating stage, and not then, except in the base of the pustule, and only when ulceration has penetrated the surface of the cutis. In many cases, even where lasting pits and deformity ensue, sloughing and ulceration will not generally be found to have penetrated through the corium to the cellular membrane beneath. I explain these points merely to set right some mistaken theory in the lately published paper.

"The application of the mask, or plaster, in the very early part of the eruption, is well insisted on. It should be applied as soon as the nature of the eruption is ascertained, and the red projections can be distinctly felt as early as the third day, and be continued *without intermission or removal* until the maturating stage has been completed in other parts of the body, a period of about five days. That this mode of treatment possesses vast power to arrest the full development of the eruption is undoubted; and I conceive so innocent a preventive merits trial by all medical practitioners desirous of preserving the appearance of their patients, and of advancing science."—*Am. Jour. Med. Sci.*

Permanent Contraction of the Fingers. By Prof. Velpeau. —Permanent flexion or contraction of the fingers depends on a great number of causes; it may arise from wounds, paralysis of the extensor muscles, anchylosis of the joints, tumors, shortening of the tendons, irregular cicatrices, &c. On the present occasion, however, I wish to direct your notice to that form which depends on contraction of the palmar fascia, or on

the formation of fibrous bands extending from the palm of the hand to the fingers, and binding down the latter in their irregular position.

Before the remarks published in 1831, by Dupuytren, followed up by those of MM. Lemoine, Mandet, Vidal, Goyraud, &c. permanent contraction of the fingers had not been much noticed by surgeons. Boyer alludes to the affection under the name of *crispatura tendinum;* Sir Astley Cooper, also, speaks of permanent retraction of the fingers and toes, which he attributes to contraction of the sheaths of the tendons of the palmar and plantar aponeuroses. Dupuytren has clearly shown that the tendons are seldom engaged in this affection, but he has not so evidently demonstrated that it depends on induration, or shortening of one or more bands of the palmar fascia.

The disease now under consideration, is characterised by the formation of certain bands or chords, which are elevated beneath the skin, and extend along a considerable portion of the palmar surface of the finger; these bands generally occupy the median line, and are attached usually to the first phalanx, but sometimes prolonged to the second or third. According to Dupuytren this disease generally begins on the ring finger, and thence extends to the little finger; it increases gradually, without occasioning pain; the patient, at first, experiences a feeling of stiffness in the palm of the hand, and is unable to extend one or more fingers; the latter soon become contracted, and, in extreme cases, the tip of the finger rests on the palm of the hand. On examination, the first thing which we notice is a chord extending along the palm and finger; if you attempt to straighten the finger, this chord becomes more tense, and it disappears when you flex the finger completely; it is rounded off in shape, and forms a kind of bridge or prominence at the metacarpo phalangeol joint. The skin is wrinkled into arched folds, extending from the middle of the palm of the hand to the base of the finger. The disease, thus progressing, may attain its maximum degree of severity without causing any pain, or appearance of anchylosis; the joints are easily flexed, but cannot be extended by the most powerful efforts; Dupuytren has seen 150 pounds weight suspended on the bent finger without producing any effect on it.

This retraction of the fingers is a very disagreeable affection; it prevents the patient from seizing any large body, and, when he closes the fingers strongly, severe pain is felt. Persons laboring under permanent contraction of the fingers, are usually those who have made violent efforts with the palm of the hand, or been employed in occupations during which the

palm and fingers are frequently pressed against hard bodies. Thus, it is met with amongst masons, ploughmen, watermen, coachmen, &c. Dupuytren mentions the case of a literary personage who was attacked in consequence of frequently using a round-handled office-seal for his letters.

What is the anatomical lesion which characterises permanent contraction of the fingers? Upon this point there exists a variety of opinions. Some attribute it to thickening and contraction of the skin; others, to spasm of the muscles; disease of the flexor tendons; inflammation and contraction of the sheaths of the tendons; some change in the joints and their lateral ligaments; finally, contraction of the palmar fascia. This latter idea was chiefly supported by Dupuytren and Sir Astley Cooper; the following case was the one which influenced Dupuytren's opinion:—An old man, who had been affected for many years with retraction of the fingers, died in the Hôtel-Dieu; Dupuytren examined the parts with great care; on removing the skin from the palm of the hand, it was found that the folds of integument completely disappeared, the palmar fascia was contracted and shortened, and several bands passed from its lower part to the sides of the deformed finger. On endeavoring to extend the fingers, Dupuytren perceived that the fascia and these bands were rendered more tense; he divided the bands, and effected, at once, complete extension of the contracted fingers; the tendons and their sheaths had not been touched; the joints likewise and bones were completely free from any appearance of alteration. From this case Dupuytren naturally concluded that the disease originated in the abnormal tension of the palmar fascia, a tension depending on contraction excited by the pressure of some hard substance on the palm of the hand. The case certainly shows that contraction of the fingers may sometimes depend on a change in the palmar fascia, but it does not prove that this is always the cause. The bands or hard chords, which, as I have already mentioned to you, are seen projecting under the skin, extend to the palmar surface of the first phalanx, and sometimes to the second or third phalanx. Now we know that the palmar fascia terminates at the root of the finger, by becoming attached to the sheaths of the tendons and ligaments; besides, the fascia does not cover the ball of the thumb or extend to its root, yet we have permanent contraction of the thumb, with the sub-cutaneous chords already noticed. Having determined, by dissection, that the palmar fascia remained sometimes intact, after the removal of these chords, I announced, in 1833, that the prolongations mentioned by Dupuytren were

not always formed by the palmar fascia; but that they seemed to me to depend on the transformation of the subcutaneous cellular tissue into fibrous bands. M. Goyraud, surgeon to the Hôtel-Dieu of Aix has proved the correctness of this opinion by his researches on permanent contraction of the finger, published in the third volume of the Memoirs of the Royal Academy of Medicine. In addition to a carefully performed dissection, M. Goyraud relates the following case, which confirms my view of the nature of this disease.

"M. Chaine, steward of the hospital, presents a remarkable example of the permanent contraction of the fingers. The three last fingers of the right hand are flexed; of the left, the four last are in the same condition. The left ring-finger is more flexed than the others; on the right side, the little finger is the one most contracted; the middle fingers are only semi-flexed. The contraction came on gradually, and without any pain. M. Chaine is now 58 years of age; when at the age of 42, he first perceived that he was unable to extend the left ring-finger completely. The latter became gradually contracted, and the affection soon extended to the two neighboring fingers. Shortly afterwards, the right little finger began to contract, and then the ring and middle fingers of the same hand. The first phalanges are now flexed at a right angle on the metacarpal bones, and the second phalanges at various angles on the first. On attempting to extend the fingers, it is evident that they are retained in the flexed position by a number of bands running from the palmar fascia to the middle part of the fingers."

In this case, the disease consists in flexion of the first phalanx on the metacarpal bone, and of the second phalanx on the first; the joints of the third phalanges are completely free.

Hence, since contraction of the palmar fascia and its digital prolongations could only influence the first phalanx, it is evident that the contraction of the second phalanx must depend on some other cause. Even in the lectures of Dupuytren we may find a proof of this. Mr. L. had contraction of the ring and little fingers of the left hand, which were completely flexed on the palm; the second phalanx was bent on the first, and the tip of the third applied to the ulnar edge of the palm: the little finger was constantly flexed on the palm of the hand also. M. Dupuytren operated in the following manner:—He commenced by making a transverse incision, ten lines in length, opposite the metacarpo-phalangeal joint of the ring-finger,

the knife divided first the skin, and then the palmar fascia. The ring-finger was now reduced nearly to its natural position.

Being desirous of sparing the pain of a second incision, Dupuytren endeavored to prolong the incision of the palmar fascia, towards the ulnar edge of the hand, but could not succeed; he was, therefore, compelled to make a transverse incision opposite the articulation between the first and second phalanx of the little finger, and thus separated its point from the palm, but the remainder of the finger was still immovable. A third incision divided the skin and fascia, opposite the metacarpo-phalangeal joint of the little finger; the benefit was very slight, and he was forced to make an incision opposite the middle of the last phalanx, when the finger at once became straight. Is it not clear, from this case, that other parts besides the palmar fascia were engaged in the disease? Do not we all know that the prolongations of this fascia are attached to the sides and not to the middle of the first phalanx?

From the facts which have just been mentioned, M. Goyraud concludes that permanent contraction of the fingers never depends on the palmar fascia; that we should avoid dividing this latter in any operation for the relief of the disease, and that Dupuytren, probably, divided the subcutaneous bands developed in the cellular tissue, and not prolongations from the palmar aponeurosis.

This opinion seems to me to be too exclusive, and I agree with M. Sanson in thinking that permanent contraction of the fingers may depend on the palmar fascia, on the skin, or more frequently on transformation of the cellular tissue into fibrous bands.

The treatment of this affection is exclusively surgical; we can expect no benefit from any other means than division of the fibrous bands by which the finger is retained in the flexed position. Dupuytren made an assistant extend the finger as far as possible, and then divided the most tense part of the band; if the finger could now be straightened completely, the operation was finished; if not, he made one or more incisions above or below the first one. When the fingers became free, he maintained them in the extended position by means of bandages and splints, for a month or six weeks; but as soon as the state of the wound permitted, he commenced movements of flexion and extension.

M. Goyraud operates in a different way: he makes his incision along the side of the band, and in the same direction with it, and then divides the band in various places, or excises a portion of it. This method has many advantages over that

of Dupuytren; the patient runs less risk of inflammation extending along the sheaths of the tendons, or palm of the hand; it allows us to commence moving the fingers at an earlier period, and leaves a much smaller cicatrix. When we have restored the finger to its natural position by means of operation, we are not to think that the patient is free from the danger of relapse. Considerable care must be bestowed on the after-treatment; the joints must be frequently moved; discutient lotions, &c., applied, and the patients must be particularly cautioned against using any hard bodies, or employing himself in any occupation by which the palm of the hand may be irritated.—*Prov. Med. and Surg. Journ.*, Oct. 1841.

New Theory of Tinea; its supposed Vegetable Origin.—A few years ago a young Hungarian physician, M. Gruby, announced, as the result of an extensive series of microscopical observations, that the cutaneous disease, known by the name of tinea or favus, is owing to the development and growth in the skin of cryptogamous plants of the genus fungus, which he has called micoderme. He tells us that they exhibit the aspect and form of such vegetable productions from their earliest appearance to their complete maturity, at which stage, if examined with a magnifying glass of two or three hundred powers, he has distinctly recognised the fruits or sporules in the interior of their cavities, as we observe to be the case in other fungi. He is of opinion that the disease is propagated by these seeds. This novel idea of the etiology of the disease has excited much attention among the French dermatologists; and it is but just to M. Gruby to acknowledge that the researches which have hitherto been made on the subject at the St. Louis Hospital in Paris, have tended to confirm the accuracy of his statements.

The treatment, naturally suggested by this theory of the disease, is that of applying caustics or whatever is calculated to destroy the vegetable substance.

M. Devergie has, during the last year, been giving an extensive trial to a solution of the acid nitrate of mercury (of the French codex): the spots become at first of a reddish yellow color, and subsequently black after the application.

M. Emery, of the same hospital, is in the habit of employing a strong solution of iodine for the same purpose, and it has appeared to answer extremely well.

In another article in the same journal, Dr. Petel recom-

mends an epilatory ointment; as, in his own opinion, the only prospect of effecting a rapid cure depends upon extracting the hairs from the diseased spots. This gentleman's experience, however, cannot well be trusted to; as he tells us that tinea is never present where the hairs have fallen out, and that the method which nature follows is always by the removal of the hair. He tells us, too, that all internal and external remedies are of little or no use until the hairs are extracted, or fall out spontaneously. It would seem, from what he says, that the empirics, MM. Mahon, have more success in the cure of the disease than any of the regular practitioners in Paris: their plan consists in applying, first, a strong lixivial soap to the head, and then an epilatory plaster, which, when removed, draws the hairs off with it.

Dr. Pelet recommends the following formula for an epilatory ointment and powder:—

Take of	Soda (of commerce) - -	60 parts.
	Slaked lime - - - -	4 "
	Lard - - - - -	120 "
	Mix together.	
Take of	Quicklime - - - -	120 parts.
	Powdered Charcoal - -	8 "
	Mix together.	

After cutting the hair very short, the crusts are to be detached by applying linseed poultices to the scalp, and washing it repeatedly with soap and water. After several days' use of these means, all the affected parts are then to be rubbed with the ointment: this is to be repeated daily. The swelling and redness of the scalp gradually subside, but without ever ceasing entirely. The pustules, the successive reproduction of which keeps up the disease, become more rare and appear only at long intervals. To effect this change, from two to three or four months are often required. When this is the case, a pinch of the powder is to be sprinkled among the hair every second day or so. The hair becomes gradually loosened, so that it can be easily detached with a fingers or with a forceps, without causing pain. When the affected parts are rendered quite bald, the treatment is nearly at an end; all that is necessary is to anoint the head with a small portion of the ointment every two or three days, and to keep it exceedingly clean.—*N. Y. Lancet*, from *Bulletin de Therapeutique.*

Case of Gun-shot Wound of the Face, with loss of a greater proportion of the Tongue, and extensive lesion of the bony structure, successfully treated; together with an account of interesting nervous Phenomena, resulting from the injury. By J. F. Peebles, M. D.—In the month of August, 1840, Washington Perkins, a middle aged man of robust constitution, but intemperate in his habits, induced by a fit of jealousy to attempt self-destruction, placed the muzzle of a fowling-piece, charged with duck-shot, immediately below and in front of the angle of his right jaw, and discharged the gun with his foot.

I saw him a few minutes after the accident; the hemorrhage was frightful, though he was composed and sitting up in bed. Upon examination I found an entire breach in the inferior maxillary bone at the point where the shot had been received of more than inch in length, involving the loss of the two lower molar teeth. Passing obliquely upwards through the mouth, the tongue was torn across in the line of the shot, all the free portion of it with the attaching frænum, completely severed and thrown forward between the front teeth. The charge passed out through the antrum about three quarters of an inch below the eye, carrying with it also the two cuspidati and their alveolar processes.

In addition to the injury of the inferior maxillary bone already named, there was a transverse fracture at the symphysis. Owing to the nature and peculiar situation of the injury, the means for arresting the profuse hemorrhage were confined principally to rest in the recumbent posture and cold applications to the head, face and neck. But the quantity of blood which had found its way into the stomach, and still continued to trickle down the throat, despite our efforts to prevent it, very much embarrassed and impeded their effects by the frequent retchings it induced. As soon as fainting came on, however, firm coagula formed in the cavity of the mouth and the orifices of the wounds, and the hemorrhage entirely ceased. In this state he was left for the night with directions for the *diligent* continuance of rest and the cold applications to the head and face.

During the night, from the frequent gratification of his intense thirst, the coagula were removed, and the bleeding partially returned, but a bit of ice in the mouth controlled it until the following morning, when all oozing was promptly suppressed by pledgets of lint soaked in a solution of kreosote applied on the bleeding surface.

His face and wounds now presented the following appearance. His mouth, particularly the lower portion of it, was

drawn to the left side; he complained of feeling a notch in the glass from which he drank, owing to loss of sensation in the right portion of his lower lip, phenomena which indicated lesion in the motor and sensitive nerves which supply the lower portion of the face. Over the inferior maxillary bone where the charge had entered there was a circular but jagged wound of over an inch in circumference, either way, extending upon the neck. On introducing the finger the fractured ends of the inferior maxillary bone were found to present a remarkable peculiarity. Instead of being shattered, and split or splintered as might have been expected from the violence of the accident and the nature of their structure, the ends of the bone were found presenting regular and transverse surfaces, as if only that plug of bone had been clearly removed which had received the violent charge, without material injury to the adjoining portion. The osseous system of this man had always exhibited evidence of remarkable fragility. He had suffered fracture of the thighs five different times, and as it is usual in individuals suffering from fragilitas ossium, the bones had in each instance united, with but little inconvenience, and with remarkable facility. Was not the regularity and favorable nature of the fracture to be attributed to this condition of the osseous system, which, doubtless, was general?

The end of the tongue was retracted and swollen, so as considerably to impede deglutition. The left antrum was exposed, while the external wound above presented the form of a triangular incision, with a flap perfectly preserved and thrown back. This was now brought down, the parts adjusting themselves perfectly together, and confined with adhesive plaster.

The portion of inferior maxillary bone between the symphysis and the breach at the angle had fallen inwards, protruding the teeth longitudinally into the mouth, and was so loose and detached as to occasion some thoughts of the propriety of its immediate removal. It was however erected into its proper position and confined as securely as the circumstances would permit, and for the few succeeding days the patient remained tolerably comfortable. After this time for the succeeding ten days alarming hemorrhage from time to time continued to recur, but by the diligent continuance of cold applications to the head and face, together with the topical application of kreosote to the bleeding surface of the tongue and cheeks, it was checked; when he ceased to suffer further annoyance or danger from this score. At the end of the second week the external wound over the antrum had healed by the first intention, and although the antrum still remained ex-

posed, the wound in the superior maxillary bone gave no further inconvenience. The tongue was also well, and healthy granulations were shooting up in the wound in the right cheek. The detached piece of inferior maxillary bone became now the most embarrassing feature of the case. As it was not fastened at either end it was impossible, such was the state of the external wound, now to adopt any means by which it could steadily be held in its proper place a sufficient length of time for union to occur; and for upwards of a month the prospect of its removal, either by necrosis or excision, was improbable. About the middle of the second month, however, it had united at the symphasis, and at once the health of the patient, which had been kept feeble by the exhausting discharge and irritation about the mouth, rapidly improved.

From this time the contraction of the muscles gradually approximated the ends of the inferior maxillary bones, and at the termination of the fourth or fifth month they became united, and the cure was complete.

The following is the condition of the man's face at this time. Considering the great loss of bone, its general contour is but little altered. The inferior maxillary bone, though somewhat shortened, and with the exception of a slight protrusion where the union at the symphasis occurred, is perfect and sufficiently strong for the purposes of mastication. The remaining portion of the tongue, doubtless from the long inactivity of its muscles, is atrophied to a mere membrane. It has well developed papillæ, however, and the taste still remains unimpaired. It affords no assistance in speech, or in mastication, (the finger being used in the latter operation to keep the material between the teeth,) but remains motionless on the floor of the mouth. The deglutition is perfect. The membrane covering the exposed antrum is healthy. . The mouth is still partially drawn to the left side, and the muscles of the lower part of the right side of the face take no part in the expression of the countenance. During laughter they are motionless, grotesquely distorting the face. But as this state of things is confined to the lower part of the face, the eyelids and muscles on the side of the nose remaining unaffected, it is probable that the lesion is confined to the lowest branch of the *portio dura* on the face. A portion of the motor branch of the fifth pair is also complicated in the lesion, as the muscles of the right cheek are atrophied, and flap, from a want of consent between their action and the motions of the jaw, in such a manner between the teeth as to prevent mastication on that side. But the *sensation* has returned in the lip. This occurred soon after union had

begun at the fracture near the symphysis, proving consequently, that the paralysis which had occasioned the loss of sensation was owing only to the pressure exerted by the displacement of bone, on the branch of the third division of the fifth pair of nerves, which emerges from the anterior mental foramen to be distributed on the lip. This man has returned to his old habits, and is frequently seen intoxicated about the streets, yet the bones of the face and the mucous membrane of the mouth continue perfectly healthy. Indeed the whole history of the case manifests a hardihood, and a strong disposition to the healthy reparation of injury in the osseous system which, I think, is quite remarkable.

PETERSBURG, March 22, 1842.

Account of a Phrenological Visit to the Penitentiary for young Criminals at Paris, made by M. VOISIN, *in Company with a Committee of Members of the Royal Academy of Medicine, on February* 17, 1839.—In addition to M. Voisin and the committee from the Academy of Medicine, there were present MM. Boullon and Pontignac de Villars, of whom the former was governor of the prison, the latter, secretary. Four hundred young criminals were examined, one by one, by M. Voisin; who, having looked at the form of each one's head, and examined it with his hand, directed him to go to the *right* or the *left*, according as his character or natural endowments appeared to be good or bad. These he subsequently divided into four classes, putting the worst in the first, the best in the fourth, and arranging in the two intermediate series those who formed a sort of *juste milieu* between the others.

Of the 700 boys originally examined, 254 were selected by M. Voisin as those whose good or evil qualities were most distinctly marked. The fourth, or best class, contained only 25, or one tenth of the whole; while 61 were arranged in the first or worst class. Of the remaining 168, 77 were placed in the third class, 91 in the second, the bad again preponderating.

M. Boullon, then gave his evidence as to the character of the youths thus classified by M. Voisin. He stated that M. Voisin's first class included, in a very great proportion, the bad characters in the house, or those whose intellectual faculties were most limited. The second and third divisions appeared to M. Boullon not to offer any striking differences be-

tween each other; but the fourth class comprehended almost all those children who were most docile, most intelligent, and most industrious. This class included the greater number of those who were employed as monitors in the school, or as overlookers in the workshops. The testimony of M. de Villars corresponded almost completely with that of M. Boullon.

A long discussion followed the reading of the report in the Academy. The two chief objections raised by the debaters were, that the testimony of the governor and secretary of the gaol was given after M. Voisin had pronounced on the characters of the boys, instead of before he had expressed his opinion; and secondly, that M. Voisin's classification implies that the intellectual and moral faculties are intimately connected, and become developed in the same proportion, while in reality no such absolute relation between mental and moral endowments exists.—*Bulletin de l'Académie Royal, Novembre*, 1841.

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REPORT ON THE MEDICAL COLLEGE.

A document bearing the above title recently appeared in one of the newspapers of this city, as part of the proceedings of the city council, and has, we understand, been distributed in the form of extras through the city and abroad over the country.

We have perused the so-called Report with all the care that such a document demanded, and with an interest not a little heightened by the circumstances under which it appeared. Its object was to procure the transfer of the Medical Institute, its grounds, apparatus, &c., from the present Board of Managers to the "Trustees of the Louisville College." Yet strange to say, the Committee from whom it emanates, almost forgetting the Managers, proceed to heap charges and opprobrious epithets on the present faculty of the Institute! However cogent these were to the Committee, and however pertinent to their design, they were deemed totally unworthy of consideration by a majority of the Council, who peremptorily rejected the report. The whole paper is conceived in the worst possible spirit, which, notwithstanding the attempted disguise, is so plain that he who runs may read. It is wholly *ex parte* in its character, and full of gross misrepresentations and misstatements. It is not our purpose, however, to enter into an examination

of this document now. The Faculty of the Institute or the Board of Managers (we are not informed which), have in preparation a reply which, it is understood, will be both full and explicit. When it appears we shall endeavor to lay such portions of it before our readers as will enable them to see the matter in its true light.

The individuals at the bottom of this movement, and the motives by which they are actuated, are alike understood and appreciated. It proceeds from certain factious and disappointed members of the profession here, who from out their holes and hiding-places have kept up a series of petty annoyances for years. But in this instance they evince a degree of *malignity* and *moral turpitude* that is rarely witnessed among men professing to be honorable. In procuring the publication of this report, *they carefully concealed the fact of its having been rejected by the City Council,* nor was such statement made until called for through the public prints. Conduct like this will meet with the hearty execration of every honest man, and with that retributive justice which, however tardy, is ever sure.

The friends of the Institute (which is the true object of hatred) have in the meanwhile nothing to fear. The blow thus intended to inflict a fatal wound will recoil on the heads of its enemies. The Faculty were never more united and harmonious; they are firmly seated in the confidence of the community, a confidence founded on their worth as private individuals, and their ability as public teachers, and are filled with the same vigor and the same enthusiasm, which in the short space of five years have carried the school from inconsiderable beginnings and made it, in point of numbers and influence, the third in the Union. The citizens of Louisville look with pride and affection on the Institute, the noble monument of their own munificence, and will mete out prompt punishment to those who attempt to check it in its swift career to glory and renown, or blight the brilliant hopes that cluster so thickly around it. C.

UNIVERSITY OF VIRGINIA.—LATIN DIPLOMAS.

Our contemporary of the Boston Medical and Surgical Journal, notwithstanding the deplorable error in his "Natural History of Fishes, &c.", and notwithstanding, too, his excessive irritability (an infirmity which in his case might be called *touchiness*), brings

to light a good thing now and then. Witness some remarks on the University of Virginia, in his Journal for August 10, from which we make the following extract.

"But there is one feature in the character of this school, so unlike that of any other amongst us, and yet so much in advance of them all in one particular respect, that not to proclaim it loudly, would do the institution injustice. It is, that 'any person of approved moral conduct may offer as a candidate for graduation, and receive the degree of M. D., without reference to the time he has been engaged in the study of medicine, or of joining the school, provided he undergoes, in a satisfactory manner, the various examinations prescribed by the enactments.' Here is an actual advancement in the mode of conferring degrees, which might be copied in other institutions, to the manifest advantage of society and the general interest of the institutions themselves. The faculty does not inquire of the candidate for their honors, whether he has studied five years or five days; but simply examines him in all that appertains to the knowledge which the statutes of the Legislature and the University declare to be the full measure of a doctor in medicine. If he sustains himself in the trial, he is at once presented with a testimonial of his attainments. Either this natural and philosophical system—which is essentially a common-sense affair —must ultimately be imitated by neighboring colleges, or we imagine the current will certainly by-and-by set in the direction of Virginia."

The feature here spoken of is entirely new to us, and possibly to most of our readers. We must give it our hearty approval. It seems perfectly consistent with that broad and liberal spirit that should characterise *all* institutions in this country. Our medical schools, in some of their regulations, are copied too closely after those of the old world, and unfortunately their models are far behind the genius of the age. If we make them, as we certainly do in some measure, the avenue to distinction, their approach should be left free and unobstructed. There is no reason why one, who, by his own unaided toil, in silence and seclusion, has qualified himself for the honors of the doctorate, should be denied them. In truth, the *reason* in the case is all on the other side.

Another matter, "essentially a common-sense affair" too, brought to mind whilst reading the article from which we have just quoted, may be mentioned here, although the connection does not seem very close. We allude to the *Latin* diplomas still retained by all of our colleges whether medical or literary; in some the degrees are even *conferred* in the Latin language. The custom is an old and time-honored one, it is true, but it has long survived the necessity which

gave it birth, if such was, indeed, its origin. We had almost said, despite the seeming contradiction, that it is a relic of barbarity, and in reference to the barbarous Latin in which the diploma is generally couched, the expression might be allowed. If ever there was a good argument in favor of this custom, there certainly is none now. In many, if not most cases, it amounts to a positive absurdity. Many times the parchment is about as intelligible to the young man receiving it, as if filled with so much Choctaw or Pottawatomie. A medical friend with whom we were recently conversing on this subject informed us that when he was in Philadelphia, several years since, it was a very common thing for the medical graduates of the University of Pennsylvania to be running about the city to the various scholars of their acquaintance in order to procure a translation of their diploma. He heard and witnessed several such applications to the late Dr. Godman, who stated that it was an annual occurrence, and that the young men gave as a reason for so doing, the mortification they should experience to return home with a testimonial that they could not read. No doubt similar occurrences take place at every medical school in the country. Could any thing more forcibly demonstrate the utter absurdity of the whole proceeding? Let us not be understood as running a tilt against classical learning; that is a very different affair. No one feels a greater reverence for what are called the classics than ourselves. At the same time, however, this does not blind us to the claims of our mother-tongue; and we hope to see the day when both British and American colleges will employ it in the formulæ of their diplomas.

C.

DUNGLISON'S AND BELL'S MEDICAL LIBRARIES.

We regret to learn from an advertisement on the cover of the last number of the first named journal, that its publication ceases from the date, ending with the excellent treatise of the late Dr. Hope on the Diseases of the Heart and Great Vessels. The universal pecuniary embarrassment of the country is pleaded by the publishers as a reason, the receipts for some time not having justified its continuance. We sincerely wish that the intimation given by them of the resumption of the work at some future day, may be realised.

The Quarterly Library of Dr. Bell is now the only periodical of

the kind existing among us. It sustains its well-earned reputation. The last number is a very valuable one, containing the several treatises of Gordon, Hey, Armstrong and Lee on the history, pathology and treatment of *Puerperal Fever and Crural Phlebitis*, with an introductory essay by Dr. Meigs of the Jefferson Medical College. Now that its contemporary has stoppcd, this should, and no doubt will receive an accession of subscribers. It is certainly one of the cheapest as well as most commodious forms of increasing a library.

C.

PRIVATE MEDICAL INSTRUCTION.

We have received, and notice with pleasure, the circular of JOHN R. W. DUNBAR, M. D., late Professor of Surgery and Surgical Anatomy in Washington College, Baltimore, setting forth his arrangements for private tuition. They are more ample, diversified and judicious than any we have yet met with, and we sincerely wish him a numerous class. His terms are $50 for one winter, $100 for a year, and $200 for the whole term of pupilage. It is in vain to deny that the private tuition of medical students in the United States is, *on the main*, humbuggery. Respectable exceptions, we admit, are to be met with, but they are by no means common. It is this lamentable deficiency in our plans of medical education, that renders it impossible for the best regulated public schools to send forth such graduates as would do them honor and raise the character of our profession. We again say that Dr. Dunbar, and all who may do like him, have our best wishes.

D.

MEDICAL APPOINTMENTS.

DR. JOHN MOOREHEAD has been selected to fill the chair of Theory and Practice of Medicine in the Medical College of Ohio, vacated by the resignation of Dr. J. P. Kirtland. Dr. Moorehead was for several years Professor of Obstetrics in the same institution; he also held at one time the situation to which he is now again called, during which period, if we are correctly informed, he lectured for the space of *six weeks* on *Coprostasis*. Should the worthy professor do

the same thing the coming winter, we hope our friend, the doughty knight of the Western Lancet, will flesh his instrument in him.

Dr. N. Worcester has been appointed Professor of *Physical Diagnosis* and *Pathological Anatomy*, a new chair created expressly for him in the same school. The faculty of this college is now complete. C.

GROSS' EDITION OF LISTON'S ELEMENTS OF SURGERY.

This book, which was announced as forthcoming in June last, is now before the public, and has met with unqualified approbation from the eastern press. We have not yet received a copy, but hope to obtain one in time to present to our readers, in the journal for October, such notice of its contents as will enable them to form some idea of the merits of the work. C.

BOOKS RECEIVED.

Our acknowledgements are due to the Committee of Publication for a copy of the revised edition of the *Pharmacopœia of the United States*.

Dr. Paine will also accept our thanks for a copy of his *Materia Medica*, received some time since.

The *Proceedings of the Thirteenth Annual Meeting of the Medical Society of the State of Tennessee* have reached us. Notices of all these will appear in the ensuing number of the Journal. By the way, can our neighbor of the Western Lancet tell us aught of the proceedings of the fifth session of the Medical Convention of Ohio? We thought the transactions were to be published. C.

THE

WESTERN JOURNAL

OF

MEDICINE AND SURGERY.

OCTOBER, 1842.

ART. I.—*Injuries of the Superior Extremities.* By W. L. SUTTON, M. D., Georgetown, Ky.

CASE I. *Gunshot wound of the axilla.*—A negro boy aged about 12 years received, March 11, 1842, the load of a shot gun in the right axilla, which passed out just behind the spine of the scapula, about two inches from the glenoid cavity. There was said to have been considerable hemorrhage at first; but when Dr. Smith saw him, in perhaps half an hour, there was none of moment—no pulse in the arm, which was ascribed to the shock. It does not appear that the left arm was examined. I saw him on the 12th, when he complained but little; no pulse at the wrist, (but in the left arm, it was distinct and irritable,) sensation imperfect, but not destroyed; temperature not materially diminished, if at all; tongue furred, whitish. Gave a dose of calomel. Reapplied a bandage on the forearm and

arm, and applied a bandage and compress in such a way as to act on the subclavian artery, below the humeral end of the clavicle. As he was five miles from town, and it was deemed important that he should be where he could receive strict attention, I directed him to be removed to Lexington, the place of his master's residence. I saw him no more, but was informed that he bore the transportation well. In a day or two mortification of the hand and fore-arm commenced. A well-marked separation between the dead and living parts took place above the middle of the arm; and about the 28th the arm was amputated at the line of separation. At this time, the original wound was suppurating favorably, and confident expectations of a favorable result were entertained. On the day succeeding the operation, he was seized with "spasms" (tetanus, I suppose), which recurred frequently until the 11th of April, when he died.

I should not have noticed this case, except to bring one fact to the notice of the younger members of the profession—a fact which I doubt not has been taught, but which I have no recollection of having seen or heard inculcated; which others *may* therefore be as little prepared to anticipate as I was. It is, that a sudden obliteration of the principal artery of a limb is not *necessarily* followed by a marked diminution of temperature in that limb. In support of this proposition, the only two cases bearing upon it, which have fallen under my observation, may be adduced. I refer to the foregoing case, in which I am satisfied that the axillary artery was either divided, or had its vitality destroyed by the load of shot; and to a case in which I tied the femoral artery about two inches below Poupart's ligament, in which neither the hand nor thermometer indicated an abatement of temperature.

Case II. *Arm torn off—injury to abdomen—death on the 11th day.*—Andrew, a negro aged 14, Dec. 16th, 1834, got his left arm entangled in the band, by which part of the machinery of a paper mill is propelled; he was drawn some ten feet to a partition wall, through which the band runs, with his abdomen directly against the end of a large beam, which ex-

tended through the wall, whereby his arm was torn off with scapula attached. I saw him probably in less than an hour—no pulse, surface cold; prescribed stimuli internally, and heat externally. In three or four hours the circulation improved considerably, when there being no hemorrhage, I dressed the wound by drawing the skin together as well as I could; but a small portion of the wound remained uncovered. Lint and adhesive strips completed the application to the wound itself; but it was judged proper to put a compress over the subclavian artery, to guard against hemorrhage, and another over the cavity made by the abstraction of the scapula, which were secured by a bandage, something in the manner of Desault's second for fractured clavicle.

19*th.*—Pulse has remained at 130; tongue acuminated, furred, white; skin dry; urine high colored, depositing a copious brick colored sediment; no appetite; complains little. To day he complains of tenderness in the abdominal muscles; and now for the first time I became aware of the fact before mentioned, that he was drawn with his belly against the end of the beam.

24*th.*—During the 19th he was troubled with bloody, slimy stools, which were attended with much griping. They continued three or four days, occasionally alternating with serous offensive stools. Much pain in the bowels occasionally, particularly at night. Pulse has been down to 104, but at present 112; tongue casting off fur, but has a rather dry and unpleasant appearance. The wound, where not covered by the skin, looks too dark; some imperfectly formed pus.

26*th.*—The serous, offensive stools continued. Occasionally he has a very severe pain in his bowels, lasting from 15 to 30 seconds. Considerable quantity of pus, of the color and consistence of cream, about the clavicle, and in the cavity from which the scapula was drawn. Over the clavicle the skin has sloughed, say two inches by three fourths, showing the bone dead. Pulse 124, small; tongue pale; sight failing. Died on the morning of the 27th.

Inspection six hours after death. Abdomen but moderately

distended. Upon opening it, the small intestines were seen of a mahogany color, with here and there a spot darker than usual. They were usually of the diameter of one and a half inches; but where darker, they were also of greater diameter, and had very distinct arborizations of bloodvessels. In numerous places, although the intestine was round before it was touched, yet upon being touched, it pitted, and did not resume the cylindrical shape. The large intestines were in general healthy; but the rectum, near its commencement, was contracted for three or four inches to less than half its usual diameter. Throughout the extent of the colon there were spots which presented the same dark color and arborizations of vessels as were observed in the small intestines. At these places also the gut was irregularly pouched. The mucous coat of the small intestines, and of the spots mentioned in the large ones, was of a rather dark mahogany color. The best idea which I can give, is to refer to Horner's Pathological Anatomy, plate 1, fig. 6, and say that the color was pretty uniform, not so dark as the deepest parts of the figure, and darker than the remainder; and to pl. 1, fig. 2, accompanying Jackson's paper on Cholera, American Journal of Medical Science, vol. 12, and say that the horizontal stripe near the middle is near the color, but not deep enough. The stomach externally appeared healthy, but was not opened. The liver healthy; gall bladder filled with healthy bile. Although the mesentery did not appear unusually dry either to the sight or touch, yet it crepitated like a dry membrane on passing a scalpel through it.

The wound of the shoulder had sloughed a good deal since the last dressing. The clavicle was entirely denuded of the periosteum for three or four inches from the humeral extremity, and two thirds of its circumference. The stitches by which the skin had been drawn together, had nearly sloughed out, although they shewed no such disposition at the last dressing. The cavity over the front of the chest, and that from which the scapula was drawn, shewed no disposition to diminish. The muscles and cellular membrane beneath the clavicle were

a good deal ecchymosed. The subclavian artery was filled with a coagulum from its origin to the point of laceration.

Did this boy die of the injuries done to the shoulder, or did he die of that done to the abdomen? I think the latter. He never complained of the shoulder, and it was only in the last days of his life, that sloughing shewed itself in the wound of the shoulder, whilst from the third day after the accident, there were considerable pain and great disorder in the bowels. It may be said that this disorder of the bowels supervened as a consequence of the injury done to the arm. Whilst I admit that such things do sometimes take place, I do not think that this was one of the cases. Every one will draw his own conclusions.

Case III. *Compound fracture of the humerus.*—W. K., aged 8, December 15th, 1830, fell from a horse, receiving the weight of his body principally upon the left hand. I saw him the next day. I found the humerus broken immediately above the condyles, the upper fragment forced through the skin on the fore part of the arm, to the extent of an inch and a half, which "two of the strongest men in the neighborhood had tried in vain to draw into place." The bone appearing healthy, a slight incision of the skin enabled me to reduce it. It was then put up in angular splints, and treated as directed by teachers. Nothing of note occurred, and in due time he was dismissed with a good arm.

Case IV. *Humerus fractured by a musket ball—artificial joint.*—Samuel Shannon, in the North West campaign in 1813, had a musket ball to pass through the humerus, immediately above the condyles. It would appear that no very strict treatment was pursued. After some weeks, he regularly bent his arm every day. His arm got well with the elbow stiff, and an artificial joint at the place of fracture. This artificial joint supplies the place both of the elbow joint, and of the rotatory motion of the fore-arm, in a very perfect manner, and he is able to do a good day's work at any kind of labor. I mention the case of this man, with whom I have lately become acquainted, to show what nature will sometimes do for

a man, and that an artificial joint is not *always* such a bad thing.

Case V. *Compound dislocation of the elbow—mortification—amputation—recovery.*—William Johnson, aged 13, fell from a height on his hands, Sept. 15th, 1839, by which the left elbow was dislocated, and the condyles of the humerus forced through the skin in front of the elbow joint. It was reduced and dressed in the straight position by Dr. Price.

16*th*. I saw him by invitation: much fever; pulse 134; tongue very much furred: fore-arm and hand cold, insensible to pricking, and perhaps a shade too dark; the capillary circulation not utterly destroyed, as would appear by pressing the skin, producing a white spot, which slowly disappeared.

17*th*.—Much the same, except that he was considerably delirious all night, and still continues so. Mortification has commenced upon the fore-arm. On consultation it was determined that amputation was the only chance of safety, which was immediately performed. He was in great danger for several days, notwithstanding amendment was perceptible in a few hours after the operation. After amputation, it was ascertained that the humeral artery, the two cutaneous nerves, and the veins in front of the elbow were torn asunder by the condyles of the humerus in their exit.

Case VI. *Dislocation of the elbow—sloughing of the fore-arm—artificial joint.*—Feb. 22, 1842, I was requested to visit in consultation W. C., aged 8, when I received the following history of the case. He fell from a fence some time in December, receiving his weight on his right hand, whereby the elbow was dislocated, the condyles of the humerus being thrown forward on the fore-arm. The dislocation was reduced and a bandage and splints applied in the bent position. Upon dressing a week after, sloughing of the fore-arm had commenced, which eventually carried away all the soft parts on the outside, back, and inside of the fore-arm, nearly to the wrist, leaving the bones naked. The wrist and hand swelled very much, and mortification of the thumb and one of the fingers ensued. At present the hand and fingers are enormously

swelled, but of a color suitable to that state; sensation is distinct. Necrosis of a large portion of the ulna has taken place, which portion has been removed; what remains is covered with red granulations. The radius gave way about the middle, yesterday. For about two and a half inches below the elbow joint, the soft parts remain sound; the elbow-joint flexible. He is represented as having remained in good health from the time of the accident; no fever or other disturbance. He is now in good health. The appearance of the arm presents to me the impression that the constitution is making a very considerable recuperative effort; and there being no circumstance to require immediate removal, it is agreed to wait on the efforts of nature. The arm was directed to be kept still, and in a case, so as to prevent motion of the fractured part.

June 14th.—I saw him accidentally; his arm in a sling without any other support; but it is said that he usually carries it in a box. The swelling of the hand has disappeared. There are two or three fistulous sores about the hand and fore-arm, and occasionally a scale of bone is discharged. The forearm is otherwise skinned over, and of good appearance; one of the branches of the radial artery has become the principal trunk. The ulna and radius have not united, but his father thinks the arm is more stiff. The elbow joint is not so flexible as it was in February. The fore-arm has shortened about two inches. No command of the fingers. The boy remains in good health.

It is too soon to say what will be the result of this case. At present the probability is that the ulna and radius will remain ununited, and that from disuse the elbow may become stiff. Yet it is possible that by proper attention both might be avoided. I certainly think that due care to prevent motion of the bones has not been used; but we dare not say that any care would have procured union.

Case VII. *Fracture of the humerus—compound fracture of the fore-arm—laceration of the external interosseal artery—arm saved.* Feb. 20, 1836, Peter, a very stout, athletic negro about 30 years old, in calendering bagging, got his hand caught between the cloth and spindle. In an instant the arm was wound round

the spindle to the axilla, and his body thrown over the spindle upon the cloth, which stopped the machine.

I found him soon after in great pain; the convexity of the back of the hand destroyed, but I did not ascertain any fracture there; the bones of the fore-arm broken in three places, at the lowest of which both had lacerated the skin; the radius still projecting; no portion of bone loose; the external interosseal artery torn and bleeding; the humerus fractured at the middle. The artery was tied, and the arm and fore-arm dressed in the usual way. Three or four hours after, he was in so much pain that it was thought advisable to remove the dressings and reapply them: now he also complained of severe pain in the chest, occasioned, I suppose, by the pressure of the body against the spindle, in being thrown over it. He was now bled, and had a decided dose of opium with calomel. He was not able to lie down for a week, and continued to suffer greatly. During that time, he took several cathartics (mercurial and mixed) and such other medicines as were thought necessary to keep down inflammation, with generally an opiate at night.

Dr. Craig and myself attended this case jointly after the first dressing, and thought it necessary to dress the fore-arm very frequently. In about five months it was well (several pieces of bone having been removed in the mean time), with no deformity in the arm; the elbow joint perfect; some deformity in the fore-arm, occasioned by a neglect or inability to keep the fragments of the radius duly separated from those of the ulna: a very imperfect rotatory motion of the fore-arm; an inability to shut the hand firmly, but with sufficient use of it to enable him to do a full day's work at spinning.

Case VIII. *Fracture of the humerus—fracture of the fore-arm —mortification—amputation—recovery.*—Peyton, a young negro man, Feb. 7, 1840, got his hand caught in calendering bagging. His arm was wound up like Peter's, with two fractures of the bones of the fore-arm and one of the humerus; no laceration of the integuments. The fractures were reduced and dressed in the usual way by Drs. Craig and Price. Three days afterwards, I saw him in consultation. Mortification had

commenced on the fore-arm, and was spreading rapidly up the arm; pulse about 100, not of a bad character; countenance not bad, but marked with suffering. Amputation was decided on and immediately performed. Mortification attacked the stump, but by judicious treatment was arrested, having made little progress.

Here are two cases very similar, the graver doing comparatively well, the less severe requiring amputation. Whence this difference in the result? From treatment? Not so; for I doubt not Peyton was treated as well as Peter. The accident found Peyton's system, constitution, body, or whatever term you choose to employ, less able to resist injury. Now as perhaps a majority of physicians upon seeing Peter's arm, might have thought amputation justifiable (if indeed not called for), and as few would have thought of amputating in Peyton's case, we are admonished not to have too little confidence in the powers of nature to resist injuries; at the same time to be prompt to interfere where circumstances require us to do so.

Case IX. *Fracture of the ulna—dislocation of the superior end of the radius—deformity.*—Aug. 10th, 1841, W. M., aged 10, presented himself, and I received this statement. Three weeks ago he was sitting on a fence, to which a horse was fastened, the horse becoming frightened, pulled the fence down with him on it, and hurt his arm; no distinct idea could be gathered as to the position in which he fell. Next day, when the arm was very much swelled, he was taken to a neighboring physician, who dressed the arm. Upon examination, I find the ulna has been broken at about one third its length from the elbow, and has united at a considerable angle, which, assisted perhaps by a softening of the ends of the fragments in the process of healing, has admitted a shortening of near an inch (measuring from the olecranon to the styloid process of the ulna), as compared with the other fore-arm. The superior end of the radius is thrown out on the outside of the tubercle of the humerus. For a moment I endeavored to force the button-like extremity of the radius into the lunated notch of the ulna: but a little reflection shewed me, that to effect that object, it would be necessary to lengthen the ulna, for which,

again, it would be necessary to break up the union of that bone. Although neither flexion nor extension of the fore-arm could be carried to the maximum, and the rotatory motion of the hand was imperfect,, yet as the motion of the elbow joint was very considerable, without pain or inconvenience, and as some rotatory motion existed; and as some uncertainty existed as to the state of the semilunar notch and the success of reducing the head of the radius (even if the ulna were broken again), and as there was danger of an artificial joint, I decided against interference.

Was this the proper course to pursue? As I am not aware that just such a case has ever been reported, I may be allowed to offer such remarks as present themselves to my mind. Dislocation of the radius is confessedly rare. I have seen, so far as I now recollect, the details of but two cases—one by Dr. Smith, of the Pennsylvania Hospital, Med. Ex., vol. 1, p. 197, and one by Dr. Hammond, Med. Chir. Rev., vol. 32, p. 285. In both of these the luxation was backwards. So far as I know, but one other kind of luxation of this bone has been recognised, to wit, forwards. A question then may be fairly entertained, was I correct in my opinion of the nature of the accident? The very remarkable prominence projecting off from the external protuberance of the humerus, as observed by sight and touch, and the distinct concavity in the end of the bone, as very evident to the touch, left no room for doubt. But, it may be said, there was too free motion in the elbow joint. There certainly was more than I should have expected; but it must be recollected, that this state of things had lasted three weeks, and the parts concerned had made some progress in accommodating themselves to their situation. Could the radius have been reduced without breaking up the attachment of the ulna? To have enabled the glenoid cavity of the radius to come into its proper position, the ulna remaining shortened, it would have been requisite to force the radial portion of the wrist joint at least three quarters of an inch beyond the ulnar portion. If this could have been effected it would have thrown the hand towards the ulnar edge of the fore-arm, beyond any allowable position. But say that a por-

tion of the excess of length should remain at the upper extremity. Then as soon as it was brought before the tubercle of the humerus, the motion of the elbow joint would be destroyed; or the outer condyle of the humerus forced backward, which, if it did not destroy the motion of the joint, would greatly injure it. Should the bone have been broken? Perhaps it would have been proper. But I repeat, that I do not know whether the cavity of the semilunar notch would remain three weeks in a condition fit to receive the head of the radius, or whether it would be found more or less filled with cellular substance, and of course presenting more or less difficulty in retaining the radius there when reduced.* In a recent case, what would be the best mode of proceeding? Any one perhaps could answer this as well as I could, but it appears to me, that extension and counter extension, with some force applied to the button-like end of the radius, assisted by rotation and straightening of the fore-arm, would be proper, and could scarcely fail.

Case X. *Fracture of the scaphoid cavity of the radius—fracture of the lower end of the ulna.*—Mrs. P., Septh 4th, 1838, was thrown from her carriage, and received her weight on the left hand and fore-arm, by which the scaphoid cavity of the radius was split, and the lower end of the ulna fractured just above the styloid process. I dressed the fracture in the usual way, and by attention, I had as much gratification as the most unqualified satisfaction expressed by the lady and her friends could give; yet I could see, that although there was no absolute deformity, still there was a want of symmetry, which I was much mortified to see in a lady's arm, especially as it occurred under my own management.

Case XI.—Nov. 1839, A. G., a carpenter, middle aged, fell about eight feet upon his hands, splitting the glenoid cavity of the left radius, and of course having a dislocation of the wrist. Being much dissatisfied with the usual mode of dressing in

* Mr. Liston, speaking of a simple luxation of the elbow joint, says: "It is almost impossible to replace the bones after three or four weeks; indeed, I have been foiled at the end of two weeks."—*Elements of Surgery, p.* 500.

such cases, and having seen a statement of Dr. J, R. Barton's mode of treating them, I determined to try it. Having the arm properly extended, I applied a roller upon the hand and fore-arm, securing a compress some third of an inch thick, and two and a half square, on the carpus and back of the hand, and a similar one on the palmar side of the radius, just above its carpal extremity, in such a way that the superior extremity of one corresponded with the inferior edge of the other. Over these I applied two thin but firm wooden splints, long enough to extend from the elbow to the points of the fingers. After a week the dressings were removed and reapplied daily pressing the fractured portion of the radius firmly so as to prevent displacement, whilst the fingers and wrist were fixed and straightened; and after ten or twelve days adding pronation and supination to these motions. At the end of treatment the wrist was found perfectly symmetrical. I hold myself under much obligation to Dr. Barton for instruction on this point.

ART. II.—*Miscellaneous Cases.* By SAMUEL HOGG, M. D., late of Tennessee.

[The following cases are taken from the Diary of Dr. Hogg, where they were recorded at various and distant periods, and probably without the most remote view to publication. They have nothing novel or particularly striking to recommend them, but they will be read with satisfaction by the physician engaged in practice. Y.]

J. M., aged 35 years, was taken on the 19th of July, 1814, with what he supposed to be bilious colic, having been subject to that complaint in former years. He had taken a cathartic when I visited him on the 22d, and was complaining of considerable nausea and burning heat in the stomach, fever, and frequent griping; his eyes and skin were yellow, his stools grey, and his urine high colored. I gave an emetic, which operated several times, but without any appearance of bile, and then moved his bowels, dislodging matter slightly bilious in its character. In the morning of the day on which I visited him,

he had had a chill. He was ordered to take 5 grs. cal. every three hours until his bowels were acted upon, and on the return of fever next day to lose 8 ounces of blood.

23*d*. Calomel had not acted. Directed cal. & jal. aa. 15 grs. to be taken immediately, and to be repeated in six hours, if his bowels were not freely moved.

24*th*. Had a chill in the morning; powders had operated three times, bringing off dark green passages; fever high, and sense of burning in stomach persistent. Bled him to the extent of 10 ounces; the serum of the blood was in excess, and appeared largely mixed with bile, the crassamentum coagulating slowly. Ordered cal. in 8 gr. doses to be taken every six hours.

25*th*. The cal. had not operated. Chill repeated; burning heat in the epigastrium continues. He lost blood again, and took sulph. sod., which acted on his bowels, inducing dark, fetid evacuations. Ordered blister to the region of the liver.

26*th*. His chill had returned; pulse still active; blister very much inflamed; general yellowness increased. He lost half a pint of blood, which appeared to consist chiefly of bile and serum. Bowels have not been moved by the last calomel; nausea distressing. Left him 5 pills of cal. of 6 grs. each, to be taken once in six hours.

27*th*. Had taken four pills, which had produced some small passages of dark green matter. Directed him to take the remaining two.

28*th*. Saw him a few minutes after he had expired. An hour or two before he died, he vomited a considerable quantity of a dark colored matter mixed with blood.

Case of Milk-sickness.—On Tuesday night, 14th Dec. 1830, I was called to see Thomas Hunt, 30 years of age, who informed me that he lived on Goose Creek, in Sumner County, and that some of his family had recently been affected with what was called milk-sickness, in consequence, it was supposed, of their having used the milk of cows laboring under the trembles. He left home for Nashville on Thursday, the 9th, and was seized, after much fatigue, with symptoms of the same disease. The symptoms were, pain in the head, chilliness, nau-

sea, irritability of stomach and vomiting. When I saw him five days after he was attacked, his bowels had not been evacuated; sense of burning in the stomach and bowels insupportable; vomiting on taking any thing into the stomach, the matter ejected being of a greenish color, and of a peculiar odor, which he said was an invariable attendant of milk-sickness. His tongue moist, clean, smooth and of natural color; general surface dry, cool, and rather livid, features shrunk, and indicating extreme distress; pulse 110 in the minute, and laboring.

Treatment. Venesection was attempted, but the sinking of his system compelled me soon to close the orifice. Ol. ricina. with 40 gt. tinct. opi. was administered, but not long retained. Fomentations of the abdomen and extremities were adopted, and pills of cal., rhu. & al. were directed in doses to move his bowels. Next morning he had taken four pills, but without the desired effect. Symptoms more alarming; patient becoming comatose, but when roused gives correct answers; great thirst; says his bowels are burning up; passes no urine. Enemata being used, he passed some very hard, dark fecal matter and bloody sanies. Cal. 30 grs. & tinct. opi. 40 grs. were administered and retained. Sinapisms were applied to the stomach and extremities. At 12 o'clock, M., 10 drops croton oil were given, and at 2 P. M. the dose was repeated without effect. Difficulty of swallowing is becoming great, pulse sinking, great chilliness. 4 o'clock. Patient is evidently sinking. Takes strong tea of rad. serpent. with paregoric as long as able to swallow. Expired about 12 at night.

Cases in Midwifery. Mrs. M., a lady of delicate habit, was taken with the usual signs of approaching labor on a Thursday morning; the midwife waited on her till Friday evening, and labor then not advancing, I was called in. I found her with considerably increased arterial action, the os tincæ but little dilated, and the child apparently large. She lost at once a pint of blood, and took 40 drops of laudanum. Her pains came on regularly, which had not previously been the case, but the os tincæ continued rigid, and the child advanced very slowly. In about five hours she was again bled to the extent of three gills, and the opiate was repeated.

The os tincæ in about three hours now began to dilate, and the membranes being found on examination to be protruded, were immediately ruptured and the waters passed off. I now found the head, which was very large, just entering the passage, and rather confined between the sacrum and pubis. Her pains began to decline, and as she was much exhausted by the labor and the officiousness of the midwife, I concluded to allow her an opportunity of taking some rest, and accordingly ordered her to be put to bed and not disturbed till necessity should compel her to rise. She slept an hour, and on waking the pains returned with great force, and in about an hour she was safely delivered of a very large boy. She had borne three children previously, with two of whom she was obliged to have assistance. The blood drawn showed on cooling considerable buffiness.

A negress of stout frame and good health was seized with labor pains on a Monday morning, having given birth to one child before. Her pains were irregular and not very strong, but in the evening, the midwife being anxious to deliver, ruptured the membranes with her finger, whereupon the pains ceased, and I was called in. I found her resting quietly, though much fatigued, and complaining of soreness, the result of excessive handling; the os uteri was rigidly contracted, and the head barely in reach of the finger. Her pulse being active, I took a pint of blood, gave her an opiate, and directed that she should be allowed to rest. She slept about three hours, and on awaking she had some pain, but as they were inefficient, I again abstracted blood from her arm. I left her at 10 o'clock at night, and charged the attendants not to interrupt her if she were inclined to sleep. In the morning I was summoned in haste by the midwife, who insisted on my opening the head of the fœtus, as she concluded it to be dead, and thought the woman could not be delivered without it. On examination I found that the head had advanced a little, but the os uteri was still rigid, and the strength of the patient being unimpaired, I drew blood a third time. In half an hour she had a call to the close stool, where she was seized with a severe pain, and on examining her soon afterwards, the head

was found nearly cleared, and the next pain expelled the child alive. I then waited a few minutes, when the placenta, after a slight pain, was safely expelled, and the woman afterwards did well.

Mrs. J. S. A., a woman of robust constitution and mother of six children, was taken on a Friday with the usual signs of approaching labor. When the midwife was sent for about 4 o'clock in the morning, the labor was well advanced, and about 11 o'clock, A. M., she was delivered of a boy. Immediately after the birth of the child the midwife proceeded to clear her, although she appeared still to be unusually large; but the placenta being firmly fixed the cord was ruptured in the effort to force it away. In the course of the next hour the midwife made frequent attempts to get it away, but failing, she became alarmed and sent for me. When I arrived I found the woman quite easy, but much alarmed with the idea of a false conception, which I soon dissipated by an examination which disclosed the presence of another child. As it was high in the pelvis I abstracted a little blood, and giving a few drops of laudanum, exhorted her to patience. In an hour, having a call to the urinal, she distinctly perceived the motion of the child. Finding that she was in no danger, the old woman having fortunately failed to dislodge any portion of the placenta, I suffered her to rest until sunset, some six hours after the birth of the first child, when, her pulse being active and the friends pressing that something should be done to expedite delivery, I took a little more blood. Next morning about 10 o'clock her pains began to return, but were weak. I gave her 40 drops of laudanum, with a tea-spoonful of tinct. camphor, which had the effect of increasing the pains, and at 1 o'clock the head was found far advanced. The child, a second boy, was born in half an hour afterwards, but was saved with difficulty in consequence of the cord having been wound twice around its neck so as nearly to stop the circulation. In a few minutes a single placenta was expelled without hemorrhage, notwithstanding that the cord had been ruptured and the umbilical vessels open for twenty-seven hours. The woman did well.

REVIEWS.

Art. III.—*Researches in Embryology, in Three Series.* By Martin Barry, M. D., F. R. S. E., Fellow of the Royal College of Physicians in Edinburgh.

These researches, though *three* in *series*, are *one* in *character* and design, constitute but a single Memoir, and are the product, almost entirely, of the scalpel and the microscope. The immediate subject of them, though not altogether new, is rare and curious. No American writer has ever seriously handled it—at least not extensively; nor, as far as we are informed, has any British one, except Dr. Barry, and the late Mr. Cruikshank.

To the physicians of continental Europe alone, especially to those of Germany and Prussia, and more recently to a few French physiologists, does the theme appear to be in any measure familiar. And even they, until the present century was somewhat advanced, had, in no small degree, neglected it, for considerably more than a hundred years—ever indeed since the time of De Graaf. But the study of it is not only revived; it is becoming active and even fashionable. Nor will the physiologists of England be indifferent to the example. For, throughout the civilized world, fashion controls science almost as much as dress. And, as an embryologist, Martin Barry, though comparatively but a young man, is already conspicuous.

Having qualified himself for the task, through reading and travel, by the acquisition of a thorough and correct acquaintance with all that had been previously written and known.

respecting embryology, that ardent and enterprising student of nature entered on the further investigation of it, by analyses and observations of his own, with a degree of industry and zeal, energy and perseverance, that have rarely been equalled, and never perhaps surpassed. And the memoir he has produced sufficiently corresponds with the time expended and the skill and dexterity bestowed on it. For its length, it is one of the most elaborate essays in the English tongue—or indeed in any tongue with which we are familiar. And if it cannot be called a learned production, the reason is plain, and much more creditable to the author, than any amount of *borrowed knowledge* could make it, though dignified by the authoritative title of *erudition*. It occupies some ground previously unexplored, and therefore new, undescribed, and unrecorded. Yet it shows that, as relates to the subject of it, Dr. Barry's literary researches have been as extensive as they could be. For, as already intimated, that indefatigable inquirer has left, as we believe, no work unexamined, in any language, that could be made available in preparing him for his enterprise, or that could aid him in the prosecution of it. He has, therefore exhibited, in his investigations of this curious branch of science, all the learning attained or attainable, either by himself or any other person; because he has exhibited all that is extant.

But however interesting and valuable these "Researches" may be to the reader who attentively studies and thoroughly masters them, they constitute but an indifferent subject for a review. The reason is plain. They are already so dense a mass of facts, that it is scarcely possible to render them more dense. They cannot, we mean, by being thrown into the form of a summary, be reduced to so narrow a compass, as to be presented in substance to the reader at a single view. And were we to attempt to give him a competent acquaintance with the memoir, and thus do justice to its author by extracts from it, our review would be lengthened far beyond the usual extent of the articles admitted into this journal. The subject, moreover, is so intricate, and, to be fairly mastered and under-

stood, requires such closeness and fixity of attention, that, curious and valuable in physiology as it is, we doubt whether it would be prized by a majority of our readers in proportion to its merit.

All we shall attempt therefore at present will be, to submit to the reader a succinct representation of the object aimed at, by Dr. Barry, and of the course pursued by him, in his attempt to accomplish it. This being done, we shall offer a few remarks on a point or two of doctrine embraced in his memoir, but not, in our opinion, correctly expounded. In this discussion we may probably offer some thoughts of our own on a topic connected with the subject of his researches, but not introduced into them. We allude to the process of menstruation.

The scheme of our author in his "Researches" is of a nature not easily defined, or characterised by words. Though strictly *elementary,* it is neither actually original, nor marked by any real profoundness of thought. Yet, pointing as it does to the very incipiency of things, it includes in it a spirit of enterprise so bold and aspiring, as to be hardly unworthy of the epithet sublime. This is the more especially true, from the consideration that its aim is to throw light on a subject which was once deemed inscrutable. And the difficulty of the project may be almost said to have justified, at the time, an entire disbelief in its practicability. But time and means, judgment, resolution and patient labor are all but omnipotent, in the affairs of man. There are few *earthly* ends that they will not attain, if, under such regulation and direction, perseverance be pushed to the requisite extent. Of this truth Martin Barry stands forth himself an eminent example.

The Doctor was not merely content to *begin his labors,* where preceding inquirers into embryology had *closed theirs.* That step, so decidedly unambitious, would have deprived him entirely of all bold and lofty pretension to originality, which he was unwilling to resign, and would have rendered him, by his own acknowledgment, a subordinate writer. He therefore as-

cended, or at least endeavored to ascend, as his point of departure, to the very fountain-head of organic existence.

Commencing with the real seminal or primitive animal matter, even before it had become organized, he has traced, with uncommon minuteness and care, and also with great apparent accuracy, its progress through all its subsequent changes of size and figure, and in all its multiplied and diversified elements, until the complete formation of the ovum, its entrance into and progress through the fallopian tubes, and its lodgment in the uterus. And there is good reason to believe, that, in his close and keen examination of these mutations and movements, he has not only made several interesting discoveries of his own, but also corrected certain errors contained in the writings of preceding inquirers, and thus doubly contributed to the advancement of embryology.

His observations, it is to be understood, were made almost exclusively on the mammiferous class of animals. And he believes (we doubt not correctly) that he has settled the question (if indeed it were not previously settled) as respects the place both of the *formation* and the *impregnation*, of the ovum, in that class. The ovary he considers the site of both processes. And that those processes occur in the same place in all viviparous, and perhaps in all oviparous animals also, is hardly to be questioned.

In alleging that Dr. Barry has *"settled"* the question respecting the site of the formation and impregnation of the ovum, we do not claim for him any thing new. We attribute to him only the full and satisfactory confirmation of the opin ion of another. By those who have paid attention to the subject it is known, that upwards of a century and a half ago, De Graaf pronounced the ovary to be the seat of both these processes. The opinion, however, was subsequently opposed by Haller and his followers, who contended that the fallopian tubes were the places where the ovum was formed out of matter derived from the ovaries, and deposited in the tubes in a shapeless and immature condition; and that impregnation was effected in the same organs. In this opinion we say Dr. Barry

not only concurs with De Graaf, but has established its truth.

For the benefit of those who may wish to repeat his experiments, it is necessary to mention, that our author conducted his researches chiefly by the dissection and inspection of female rabbits, during the period of their rut or sexual desire, especially just before and after the act of copulation. And he was so fortunate as to collect evidence still farther confirmatory of the fact, well known before, that the rutting or amative season of the mammaliæ, is uniformly accompanied by an augmented vascularity of the ovaries, and other parts of the generative system, as well as by a preternatural accumulation of arterial blood in them. This phenomenon in the females of the inferior animals, as will hereafter be made more fully to appear, is analogous to menstruation in woman, and is no doubt designed to subserve the same purpose—the preparation of the generative organs for the process of impregnation, and, in woman, for the continuance of them in that condition.

As a farther element of the history of the memoir we are examining, it may be mentioned that it first appeared as a communication to the Royal Society of London, and was printed originally, and we believe exclusively, in the Transactions of that distinguished body. The plates contained in it are very numerous, of great diversity, and highly finished. They are also of great value, as without the demonstrations they afford, the text of the work would be unintelligible. Such, as we are told, was the estimation set on the memoir, by those appointed to judge of its merit, that it opened to its author the door of the Royal Society, of which he is now a member. So much for all the general remarks we deem it requisite to make on the memoir.

We have alluded to a point of doctrine maintained by our author, in the soundness of which we do not concur with him, and which we have promised to make the theme of a brief discussion. It is contained in the following extract from the memoir.

"As to the particular *locality* in which the ovum becomes

susceptible of development [*is impregnated*] physiologists are not agreed. Some maintain that it acquires this susceptibility before it leaves the ovary; others that the change is not effected until after its expulsion from that organ.

"It is not my intention te discuss the question whether contact of the seminal fluid with the ovum is or is not essential to impregnation. Yet perhaps it may be proper to refer to the *possibility of that contact*, while the ovum is still in the ovary, that having been denied."

To demonstrate this possibility, and we may say, to show his *belief* in the *actual occurrence* of the contact which he pronounces *possible* (for such belief he virtually avows) our author continues.

"In *seventeen* out of *nineteen* instances of the rabbit, though the parts were generally examined while warm [*immediately* after the death of the animal—and also soon after coition] I was unable to discover spermatozoa in the fluid collected from the surface of the ovary. In the other *two* instances, however, spermatozoa, or at least animalcules exactly like those I had been accustomed to meet in the uterus and vagina, were really found in the ovary."

That the ovum is impregnated as well as formed in the ovary, is a point of doctrine, in the truth of which we have long believed, and have often defended it, for the most part *orally*, but also through the press. The striking changes which take place soon after coition, in one or more of the ovarian vesicles, previously to any discharge from them, furnish abundant evidence of the fact. But that the semen masculinum, whose influence is essential to impregnation, makes its way to the ovary, and, in its formal and entire character, comes into immediate contact with the ovum, or containing vesicle, we deem not only doubtful, but altogether incredible. And though we do not pronounce the event *impossible*, we regard its improbability as of the highest order—of the same order indeed with that of the spontaneous *ascent* through the air of a mass of iron, or the *descent* of a volume of hydrogen gas

without any compulsive force, the atmosphere being tranquil, and in a natural condition.

The cause of our disbelief, moreover, seems to us reasonable and plain. We consider every positive and well-established fact of the case as testifying strongly against the hypothesis. Nor, of course, if this be true, is there, or can there be a law of animal action in favor of it.

Some of the facts alluded to shall be presently cited. Previously however to this, we shall specify the principal modes, by which the semen masculinum is supposed to be brought into contact with the ovarium, and state our objections to them.

1. Some physiologists (perhaps a majority of those of the present day) contend that that fluid is introduced into the uterus, during coition, *vi jaculationis penis masculini;* and that thence, by some *secret* spring of movement (for none such has yet been discovered, nor scarcely even fancied) it glides or hurries forward, enters and passes along the fallopian tubes, issues out of them again, and finally reaches the surface of the ovarium. Nor is its journey yet terminated. It penetrates the ovum, and comes into immediate contact with what our author terms the "germinal vesicle;" and there the work of impregnation is completed.

Such is the darksome journey—half by compulsion—half spontaneous—supposed to be performed by the semen masculinum, in quest of the place of action which awaits it. And that it is an arduous one, the most sanguine of its advocates must acknowledge. To us the series of obstacles opposed to it, even when the generative organs are in a sound and perfect condition, appears to be insuperable. And under the influence of certain derangements of them, which not unfrequently occur, without preventing the process of impregnation, the impediments to the passage of the fluid toward the ovarium are so much augmented, that, in her efforts either to remove or surmount them, even fancy herself, from a lack of expedients, must falter and fail. Of the difficulties which thus present

themselves, when the generative organs are in a healthy condition, the following are a few.

The axes of the vagina and the uterus do not so correspond in direction, as to meet and constitute a straight line. At their junction with each other they form an angle, which, though far from being acute, effectually prevents the semen masculinum from being projected into the *os tincæ*, and through the neck and strait, into the cavity of the uterus. The semen, when ejected in coition, may strike the uterine cervical extremity; but that it cannot enter into and pass through it, every fact bearing on the subject conclusively shows.

In the virgin state of the female generative organs, the rugæ formed by the lining of the vagina constitute another obstacle to the projection of the semen. Nor is this all. The viscosity and weight of that fluid, united to the adhesiveness of the mucus spread over the lining of the whole canal forms a further very serious obstacle to its projectility through a crooked and very narrow opening, or rather through no actual opening at all (for the sides of the canal are in contact) into the cavity of the uterus. In truth it appears to us, that a more impracticable and hopeless project can hardly be imagined than that of introducing the seminal fluid, during coition, *vi jaculationis*, into the uterus.

But admit the difficulties of the introduction to be vanquished, very little is yet done toward the completion of the enterprise. At least the most arduous portion of it remains to be performed. The fluid has lost the whole impetus or force of movement received during coition from the male organ. Nor does there exist, in the surrounding parts, a single organ or portion of machinery, to communicate to it a fresh impetus. It must first, by some instinct and locomotive power of its own, grope its way to the fundus uteri, there search out the scarcely more than microscopic orifices of the fallopian tubes, enter them, and then crawl along those lightless canals, until it shall finally arrive at the ovarium, there to aid in the perpetuation of our race. We say *our;* for our observations relate to hu-

man impregnation. Yet are they applicable to that function on a much broader scale—probably to *universality itself.*

But, according to the tenets of the hypothesis we are examining, the travels of the semen masculinum are not at an end, even when it has perched on the ovarian vesicle which contains the ovum. It must pierce through the parietal membrane of the vesicle, as well as through the proper membrane of the ovum, before it can operate effectively on the "germinal vesicle" of that body. And, if we mistake not the hypothesis, on that it must operate, else impregnation does not follow. *The contact must be complete.*

That the discussion may be conducted, on our part, with the utmost fairness and liberality, we again admit what we consider inadmissible, because we conscientiously believe it can never occur. We concede that the seminal fluid has not only reached the fundus uteri, but that it has made its way into the minute orifices of the fallopian tubes. But how is it now to continue its journey, and finally reach its place of destination? The tubes can by no possibility conduct it there. They are in the condition of a broken-down bridge, which positively refuses to the traveller a passage. To drop figurative language, and express ourselves in that of our profession, the fallopian tubes are in a *flaccid condition.* But when in that state, they do not, by a considerable distance, extend to the ovaries, and cannot therefore convey to them and deposit on them aught they may contain. To perform that office they must be in a state of *vital erection*, prepared for efficient functional action. For surely our opponents will not contend that the seminal fluid, by being merely admitted into the fallopian tubes, passes through them without their aid, and equally without their aid, makes its way to the ovary. Yet notions no less preposterous have had their advocates. Nor are we by any means confident that fancies as visionary, respecting impregnation, are not now entertained and defended by individuals who rank with the gravest and wisest of our philosophers.

Does any one ask us how we know that the fallopian tubes

are flaccid during sexual intercourse? To this our reply is prompt and positive. We know it because it has been ascertained by observation. And Dr. Haighton, one of the first physicians of his day, was the experimenter and observer. He killed a female rabbit, when in full rut, *immediately after coition*, and the fallopian tubes were completely flaccid. Nor did they exhibit the least sign of having been recently rigid, or injected with blood. He then proceeded to kill other female rabbits, also in rut, at every hour after coition, until the *ninth* hour, when incontestable signs of impregnation already presented themselves; and the fallopian tubes were but just beginning to prepare themselves to pass into a state of rigidity. They were beginning, we mean, to be preternaturally injected with arterial blood. And wherefore were they so? The cause may be easily rendered. Their action was to be soon required to receive, convey to the uterus, and there deposit, the contents of some ripe ovarian vesicles, which were preparing to burst, and commit to the fimbriated extremities of the tubes the matter within them. These experiments were several times repeated with the same results. Nor have either these, or the many other experiments of Dr. Haighton respecting impregnation, though cavilled at and inveighed against, ever been subverted or in the slightest degree invalidated by the experiments of any other writer on the subject. Indeed they cannot be subverted, for the best of reasons. They have been conceived with judgment—and accurately performed; and their foundation is truth.

To speak of this matter plainly and irrespectively, and apply to things the terms that truly expound and graphically characterize them. The circumstances of the case compel us to regard as essentially preposterous that hypothesis which contends that the semen masculinum passes into *positive contact* with the ovarium. Once more we emphatically ask, and solicit to the question the serious and deliberate attention of physiologists—by what means is the passage effected? or, with the present organization and endowment of the parts concerned, by what means can it be effected? And we challenge a

calm and philosophical reply—a reply abounding in fact, reason and argument, and free from conjecture, assertion, and fancy.

That by the projectile power of the male organ, in the act of coition, the seminal fluid can be thrown into and through the uterus and fallopian tubes, and thus be brought into actual contact with the ovarium, we again declare to be *physically impossible.* Admit, we repeat, that it can be thrown into the uterus, *vi jaculationis;* it can be thrown no further. As far as the original impetus is concerned, there it must stop. How then is it to be put in motion again, and conveyed to the ovarium, its supposed place of destination? The uterus most assuredly has no apparent fitness, in either structure or habit (as far as we are apprized of its habits), to conduct it thither, in a course of motion directly the reverse of that which its customary action produces. For it need hardly be remarked by us, that the usual, we believe the *universal* direction of the action of the uterus is from the fundus to the neck—not from the neck to the fundus. But did it convey the seminal fluid to the openings of the fallopian tubes, the latter must be the course of action taken on by it; because no other would produce the desired effect. Nor were the fluid actually introduced into the uterine end of those tubes, do they appear to be in the slightest degree qualified to convey it to the ovarium. Why? Because the action suited to such conveyance would, in its direction, be exactly the reverse of that which they are positively known to perform. Matter has been often found in them, in its passage from their fimbriated extremities to the uterus, but never journeying in the opposite direction. Never has any substance been detected in them *certainly* passing from their uterine to their ovarian ends. All allegations to that effect are but unsubstantial guess-work.

It is definitively known, we say, that the fallopian tubes carry matter from the ovarium to the uterus. Were it equally certain that, *when in a healthy condition,* those same tubes carry matter, by their *natural* action, in the opposite direction, the fact would constitute a phenomenon *unique*, as we

believe, in human physiology. We remember, at present, no other instance, in the system of man, where a tube, by its natural and healthy action, is alleged to convey matter, either solid or fluid, in opposite directions, at different times. The healthy functions of our systems are coincident and harmonious. If, in any case, contrariety or clashing occur in the economy of our bodies, it is the result of a condition of things neither sound nor natural. The mere fact that the cystic bile *twice* traverses the cystic duct—first toward the gall-bladder, and then toward the duodenum, can with no force or justice be cited in contradiction of the general truth in physiology here contended for.

Will any one allege that the spermatic animalcules—the *homunculi* of former writers, and the *spermatozoa* of those of the present day—will any one fancy or even dream that those microscopic specks of matter, containing within themselves the impregnating and quickening leaven of vitality and being, and possessed of muscular or some other sort of locomotive powers, crawl, and grope their way, from the vagina, or the neck of the uterus, through the worse than Cretan labyrinth of the generative apparatus, until, without either chart, compass, or clew, they arrive at the ovarium, enter into the ova, complete the work of impregnation, and, becoming themselves embryos first, and infants next, are ultimately developed and matured into adults? Will any one peril his reputation for intellect, by avowing himself the advocate of a notion like this? We think not. Yet would the fancy, wild and visionary as it is, be just as consistent with reason and common sense, as any of the elements of what may be called significantly enough, the CONTACT HYPOTHESIS—of that hypothesis we mean which, in its scheme of expounding things, attributes to *contact* an influence and a consequence far beyond what it possesses—which, in particular, doggedly maintains, that of bodies in contact the mutual action is not only much more certain, but also much more *explicable*, than of those that are not in contact—and which, in fact, virtually contends, that their mere *being in contact* is of itself a satisfactory reason *why* bodies

do act on each other, and their simply *being apart*, a sufficient season why they *do not*. But of this *contact-hypothesis* due notice will be taken hereafter; while, in the mean time, we shall proceed in the discussion of our more immediate subject.

The foregoing are some of the difficulties to be encountered by those who would conduct the semen masculinum to the ovarium, the genital organs of each sex being in a natural and healthy condition—such a condition, we mean, as offers no impediment to their most efficient action. And the difficulties of the case are greatly augmented, in magnitude at least, if not also in number, by various derangements which occur at times in the organs, while the power of propagation remains, *certainly undestroyed*, if not unimpeded. Of such derangements the following may be noticed.

The *projection* of the seminal fluid into the uterus is augmented in difficulty by the *truncation* or shortening of the penis, by the loss of a large portion of its entire body, as well as by strictures of the urethra, which scarcely allow even the urine to pass. And such *projection* is rendered impossible by anomalous openings of the urethra, not at the point of the glans, but on the underside of the penis, *behind the glans*. Yet, while laboring under these embarrassments, men have often had children born to them. Instances of the kind have fallen under our own notice, while, as related to the fidelity of the mothers, no shade of suspicion was entertained.

But the most hopeless obstacle to the passage of the semen masculinum to the ovarium remains to be mentioned. It is the entire occlusion of the vagina, the os tincæ, or the neck of the uterus. This impediment, absolute while it continues, is effected in several ways; by imperforated hymeneal membranes, which have unquestionably existed; by morbid adhesions, the product of inflammation, which have also existed: and by tumors arising from various causes.

That under the obstruction to the passage of the seminal fluid into the uterus, offered by these anomalies, impregnation has been effected, we are forbidden to doubt. Testimony to that

effect is transmitted to us, on authority as high and valid, as any which the annals of our profession afford. Among the individuals who have recorded themselves as witnesses of such occurrences may be mentioned Harvey, Morgagni, Hildanus, Ruysch, Simpson, and Haller—men whose veracity has never been impeached; whose competency to investigate and judge has never been questioned; and whose contributions to medicine have immortalized their names. Nor is this all. Several strongly attested cases, of a more recent date, are publicly recorded, and have been reported to *ourselves* in private letters from individuals both of intellectual distinction and high moral standing; and we have in person witnessed a case, on which we could put no other construction. But to close our descant on these difficulties, all of which are formidable, and some of them insuperable, and pass to another subject.

Perhaps there is no exercise of the mind in which a greater display of *false philosophy*, or as it might well be denominated *philosophical folly* is made, than in that of stubborn adhesion to what we have called the CONTACT HYPOTHESIS. And more or less abundantly that hypothesis throws its influence into every profession, and every department of human research.

The medical adherents to it, who constitute a large majority of the Faculty of christendom, can neither comprehend, nor apparently believe in the action of medicinal substances, except they be in some way, in their mode of employment, connected with *contact*. Hence, if such substances prove in any degree effective, they are believed to be dissolved or mixed in the blood, which conveys them directly to the deranged portions of the body, and thus enables them, through immediate contact, to act on and cure or relieve their disease.

For this high efficacy and influence of contact these gentlemen contend, without having, in many cases, the slightest evidence that such contact exists (for the medicinal substances which they believe to be mingled with the blood cannot be detected there), and without forming a *definitive*, or, even tolerably *exact* idea of the import of the term.

Strictly speaking we are hardly if at all authorized, *as philosophers,* to say that any bodies, of which we have definite knowledge *are in mutual and positive contact.* We can pronounce them only *near to each other.* Why? Because bodies of any appreciable size can, by certain powers and processes, be brought still nearer to each other, than they are in what is called a natural state. And the same is true of even the minute constitutional particles of matter existing in masses. They can be still more closely approximated to one another. In other words, each mass can itself be more consolidated, which implies an increased proximity of its particles. As relates to bodies not brittle, more especially if they be possessed of life, this is certainly the case. Their constituent molecules can be brought nearer to each other than they are in their usual condition. Of all masses of fluid matter, quicksilver itself not excepted, the same may be affirmed. By compression and otherwise the nearness of their elementary atoms to one another may be increased. A few further remarks on the term *contact* may aid us therefore in rendering our views, in the present instance, the better understood.

In preciseness of expression, contact is undefinable; because it has no definite and positive meaning. It is an abstract or general term bearing relation to space, and indicating degrees of proximity that may be pronounced almost innumerable, and therefore exceedingly different from each other under different circumstances, and in different cases. It might be called, with sufficient significancy, the PROTEUS of the English language. Were we to attempt to affix to it any settled definition, after having pronounced it "undefinable", we would say that it signifies (if indeed it can be made to signify any thing) the shortest space that does or can exist between two bodies—or the shortest space we can measure in fancy. Like the notion of the infinite divisibility of matter, it is an abstract truth, but a practical fallacy. On this abstruse and interesting point we shall only add, at present, that notwithstanding all that is said about contact and its effects, it is almost certain, that between solid and ponderable bodies, it *has no existence.* Such bodies appear to be kept

apart by a subtle, elastic and imponderable aura; which extends to all places, and penetrates all things, but of which nothing is known except from its effects. Might we coin a word suitable, as we think, to the present occasion, we would say, that, as relates to both massses and molecules of *ponderable* matter, this *imponderable aura* seems to act the part of a universal *betweenity*—a substance which lies between them, and so far separates them from each other, as to allow them room for some *sort of action*—for we cannot doubt that, more or less, *every atom of matter acts a part* in the scheme of creation. In a system marked by positive and *consummate wisdom and perfection* (and such is and *must* be the handiwork of an ALL-PERFECT BEING), there can be no *idleness*. For idleness is essentially *imperfection*.

If all this be true then (and the refutation of it is fearlessly challenged), how fallacious are the views, and how futile the labors of our *contact*-philosophers, who conceive themselves in possession of a talisman, competent to the solution of some of the most obstinate difficulties in physical science! And that talisman is *contact*. And that contact is nothing more than an *imaginary or supposititious* relation. Yet do those mistaken inquirers seem really to believe, that if they can bring two bodies together, and place them in what they call *contact*, they fully understand and can clearly expound the reason and mode of their operating on each other; and that if the bodies be *not* thus brought together, they cannot operate on each other at all.

Such we say appears to be the groundless notion of those votaries of contact, while it would be easy to show, had we leisure to dwell on the subject, that it is by the mutual influence of bodies *not in contact*, but remote from each other, that the economy of nature is chiefly carried on. In proof of this we might refer to the organization and movements of the heavenly bodies, which are maintained and regulated exclusively by *remote agency*. The agents in this magnificent and sublime department of creation, are termed gravitation and repulsion, or rather the principles of those high and important forms of action. And to the natural and effective operation of those principles, distance between the bodies acting on

each other is essential. Were the several orbs which belong to the solar system, and those still more remote from us in space brought into contact or close proximity to each other, there is reason to believe that the economy of creation would be disastrously changed; and that little short of general ruin would ensue, and a new order of things succeed to the wreck.

When we advert to terrestrial matters, and take a survey of them, phenomena similar in character, and governed by similar principles of action, every where present themselves to us. In confirmation of this we may again refer to gravitation and repulsion, in all their multifarious effects and bearings, and also to the workings and manifestations of magnetism, electricity and galvanism. In the production and play of the phenomena proceeding from these agents, bodies remote from each other are in all instances and necessarily concerned; and in none of them is contact either indispensable or useful. On the contrary, it would derange and confound the whole scheme of action, now so beautiful, harmonious, and perfect. And there is reason to believe, that the same all-pervading, subtle, and ever-active aura, to which reference has been already made, is essentially influential in the control of the pageant.

But we have not yet closed our remarks on the influence and operation of distant bodies on one another. Far from it. The most striking and appropriate to our present purpose are yet to come. And they will bear relation to living matter, especially to the human system, in the performance of its functions in both health and disease. The following are a few of the facts by which our position is sustained.

When wine or other cordial and exciting articles of food or drink are received into the stomach of persons exhausted by long fasting, fatigue, or otherwise, they throw their influence *instantaneously* throughout the entire systems of those who swallow them, and refresh and invigorate the most remote in common with the most proximate organs of their bodies. And this they do long before it is possible for them to reach

in substance, through the circulation of the blood, the portions of the system which they thus revive. Certain poisonous substances in like manner, if introduced into the stomach, or even applied to the lips, tongue, or eyes, act with equal suddennesss on the most distant parts of the body, and produce death. Nor is this all. Deleterious impressions made on the skin by cold, burning, or in any other way, derange by sympathy the internal or remote parts of the body as certainly, and almost as speedily and seriously, as if they were made on the sympathizing organs themselves. And irritation on the lower extremities is known to have produced gastric spasms, tetanus, mental derangement, and death. Innumerable other instances moreover of a like description might easily be adduced.

Actual contact then between bodies, whether it be positively producible or not, is not necessary to their acting on each other. As far as we are informed on the subject, this is true of the entire scheme of material creation. But it is preeminently so, as relates to the economy of man, and other forms of living matter, in which all action, whether natural or unnatural, healthy or morbid, is the product of *stimulation* —the product, we mean, of an influence wholly unknown to *dead matter*, and of which dead matter is unsusceptible—the product of an attribute peculiar to *living* matter, and which is as entire a stranger to chemistry, as chemistry is to mathematics or theology. And it is perfectly well known that for the production or change of action in a part, whatever may be its character, *remote stimulation* is as efficacious as *proximate*. Our meaning will be understood to be, that an organ may be, and often is, as fully and effectually excited and thrown into action, by suitable impression on a distant organ, as it can be by impression on a contiguous one, or even on itself.

On the authority of the foregoing facts, then (and many others we say of like import might be cited), we confidently repeat, that, to whatever extent appearances and even authorities may testify to the contrary, *contact* between bodies

is not indispensable to their highest and most efficient degree of action on each other. In many cases it is unfavorable to such action, and in not a few entirely preventive of it. In a special manner it furnishes no facility in detecting the causes of action; or in giving any correct philosophical exposition of it. It enables us to assign no reason, why, on scientific grounds, the action may not have as certainly occurred without contact as with it.

It has long been matter of surprise to us, especially as relates to living beings, that this truth, obvious and incontestible as it is, should be so completely overlooked. For overlooked it is by almost every one—the cultivated and enlightened portion of the community forming no exception to the oversight, any more than the uncultivated and ignorant.

The universal impression is, that, provided an agent, whether salutary or deleterious, be brought into contact with any organ or part of the body, all difficulty, as respects causation is removed, and the reason why such agent produces a given effect on the part it touches completely understood. And *mere contact* is the talisman honestly though heedlessly believed to solve the difficulty, and render obvious to every one the entire cause of the action produced, and the result that follows. Yet to the philosophical inquirer and the clear and able thinker a moment's serious and strict examination cannot fail to render it certain, that the simple contact of bodies does no more toward the disclosure of the cause why they act on each other, nor toward the exposition of the mode of their action, than would their remoteness from one another do, or than would any other practicable posture, bearing, or condition of them do. For we repeat what we have heretofore stated in substance, that as regards a fitness to disclose the cause of the action of one body on another, *contact* and *remoteness* are in a like degree *nullities.* It is the *properties* of bodies, not their *positions* or mere *exterior relations* that fit or unfit them to act on one another. So much for our general doctrine. Now for its application to the subject of our inquiry.

Notwithstanding the foregoing truths, which, though we

may *deem* them incontrovertible, we do not *dogmatically proclaim* them so, nor claim or even solicit for them aught save a deliberate and impartial examination—the stricter the better—notwithstanding these considerations, most physiologists seem to think, that if they can succeed in showing that the semen masculinum, positively finds its way to the ovarium and touches it, they have explained satisfactorily the process of impregnation. The mere coming together of the male and female products appears to be to them sufficient for the purpose. It would seem, in their estimation, to explain to them the mode of operation of the two substances on each other, and to reveal to them the causes of the events that succeed. So deep is the deception which these physiologists practise on themselves, as well as on the public, by the negligence, and superficialness of their mode of research! Those gentlemen moreover under the influence of a quixotic spirit, seem pleased with the pastime of creating or imagining difficulties, and placing them in their own way, that they may display their prowess and dexterity in attacking and vanquishing them. In the present case they have two difficulties—one of them *artificial*, being needlessly manufactured by themselves, and the other erected by the hand of nature. The first is the difficulty of conducting the semen masculinum to the ovarium; and the second, the difficulty of knowing how to dispose of it on its arrival, and to understand and expound the office it has to perform. Nor would the latter difficulty be at all diminished could they actually bring the seminal fluid to the ovarium, and diffuse it over the surface of that organ, or even introduce it into its interior. They could as easily discover and explain its mode of operating and producing impregnation, when at a distance, as when thus in contact with the ovarium, or even when within it. Their hypothesis is in no way or degree benefitted therefore, by the more than herculean task of conveying the seminal fluid to the female organ, where they are ambitious to place it.

We, on the contrary, content with encountering a single *natural* difficulty, create unnecessarily no *artificial* ones.

Hence we decline the needless, useless, and impracticable labor of conducting the semen masculinum to the ovarium. We have therefore to contend with only the simple natural difficulty of expounding the influence and operation, *by sympathy*, of the semen masculinum on the ovary and its contents. And that we make no effort to expound, convinced of the impracticability of the task. We only admit the process as a fact, and illustrate and sustain, as best we may, our views in relation to it, by means of analogy. We repeat however that there is no more difficulty in accounting for the sympathetic influence of the semen masculinum on the ovarium, when deposited at a distance from it, than there would be, were it applied immediately to its surface—or even introduced into an ovum and brought into contact with the "germinal vesicle." And to make good their enterprise, we say again, the *contact-defenders* must thus introduce it, else their labor is lost, and their scheme defeated. Of the sympathetic theory of impregnation more will be said hereafter in detail.

Another class of physiologists equally bent on bringing into contact, in the work of impregnation, something of the masculine, and something of the feminine gender, have recourse to their fancy, and, by the creativeness of that, produce what they denominate a seminal aura. This they derive from the semen masculinum deposited in the vagina, and conduct it through some route (no matter what) to the ovarium, where, by contact with the ovum, or rather by making its way into it, it effects impregnation. But this aura exists only in the notions of visionaries. Indeed the whole hypothesis is but a mere creation of fancy, and shall therefore be dwelt on no longer, as a subject of discussion.

But we are not yet done with this phantom-tale of *contact.* A third set of physiological schemers, resolved on sustaining their notion, and making it subservient to a favorite hypothesis, convey in imagination the semen masculinum to the ovarium by a third and more circuitous channel. They contend that that fluid, being absorbed from the vagina, where it is deposited, *during coition*, enters into the blood vessels, mingles

with the blood, and, by that fluid, in the course of its circulation, is brought into immediate *contact* with every part of the body, and of course with the ovarium, where it effects impregnation.

To every one who may seriously reflect on it, the fallacy, not to say absurdity of this hypothesis, cannot fail, we think, to be obvious and glaring. Were it true, the case would be alarming to virtuous women; because the effect might be the peopling of the world with swarms of *illegitimates.* The entire mass of blood in the impregnated female would or at least *might* be imbued with the male seminal fluid, to such an extent, as to render it contagiously operative on other women as well as on its owner. In such a state of things, simply to introduce into the veins of an unimpregnated female a portion of the blood of a pregnant one, would produce in the former a condition similar to that of the latter. In plain terms it would impregnate her also. Thus might animal reproduction be promoted, like small-pox, or the vaccine disease, by simple inoculation. At least, ludicrous as the fancy may be thought, we perceive no good reason why the event might not occur, could the whole mass of a woman's blood be so saturated with semen masculinum, as to impregnate herself. But we must dwell no longer on so dreamy a conception. There exist many other hypotheses of impregnation, founded also on the notion of *contact*, all of which are untenable, and some of them, especially that of Buffon, wild even beyond the fictions of romance. In the citation and discussion of them therefore we must consume no more time.

Having thus avowed our dissatisfaction with all the *contact-hypotheses* on impregnation, were we asked, in what theory we are ourselves believers? we would reply; in that which *rests on the doctrine of sympathy.* But we shall speak of it cautiously. Our meaning therefore is, that we give a preference to that over all other doctrines, without positively averring it to be true, beyond contradiction, doubt, or cavil.

Of this theory the ablest and most laborious advocate and sustainer was Dr. Haighton. And to us his experiments on

the subject are as satisfactory and conclusive, as are those made by any inquirer, or on any physiological theme, that can be named.

It is a matter of regret to us that a want of room forbids us to submit a detailed account of those experiments to the scrutiny and judgment of our readers, and compels us to rest content with laying before them, in general terms, a brief exposition of the principles of the theory. And that this exposition may be the clearer and more definite in its elements, and the better understood, a few preliminary remarks will be offered.

That no mistake may exist as to the exact meaning of sympathy, as to be here interpreted, it is requisite for us to observe, that it is a term employed to denote the fact, that an impression made on one part of the human body, when possessed of life, produces excitement and action in another part, near or distant, as circumstances may direct. And the kind of action excited depends chiefly on two contingencies—the nature and character of the exciting body—and the organization and nervous endowment of the part that acts. Something also depends on the condition of the body as relates to health. In a particular manner the production of a specific effect requires the influence of a specific cause.

A well known fact in physiology, interesting in itself, and important to be borne in mind in the present discussion is, that sympathy in woman is much more vivid and powerful than in man, and therefore capable of the production of a deeper result. And another fact equally well known is, that the *genital* system of woman controls more powerfully her *general* system. To these facts are to be added another of equal certainty—that each portion of the female genital system sympathizes strongly with every other portion of it; and that it is these cords of mutual sympathy which *constitute it a system.* These positions are to be regarded as so many fundamental truths in physiology, and as such to be remembered in the sequel of this discussion.

Another important physiological truth to be also borne in mind, on the present occasion, is that the ovarium is essentially a glandular body, capable, like all other glands, of producing out of the blood something which the blood itself does not contain in a formal state. By means of this *creato-constructive* power (might we compound a term expressive of our own opinion) the ovary gives being to the ovum, in all its elements, which, *substantively* but not *formally*, had been previously in the blood.

A farther truth of interest and moment to us is, that, as already stated, vital action is the result of *stimulation*, and without stimulation of some sort could never be produced. The notion that it is or can be the result of any form of chemical agency, is as utterly absurd as would be the ascription of mechanical products to *moral means*.

Of stimulation it may be correctly enough said that there are two classes or kinds—*proximate* and *remote*. Of these the former arises from impressions made immediately on the stimulated part, while the latter is the product, *through sympathy*, of an impression made on a distant part. And in various instances, each sort of stimulation excites *secretory* and *nutritive* or *formative* action in perhaps an equal degree. As a general rule, however, there is reason to believe that remote stimulation is most efficient—ministers, we mean, to the performance and steady maintenance of the greatest number of important functions. This truth, too, our readers will do us the favor to bear in mind.

To the production of the ovum, both secretory and formative action are essential. By the former the requisite materials are prepared from the blood. By the latter those materials are constructed into the ovum. We shall only add, in this place, that secretory and formative action are excited by remote stimulation, as certainly and effectively as by any other kind—very often much more so. In the following instances the truth of this position abundantly appears.

1. *Secretion of tears.* This secretion is excited by those impressions on the brain (for they are nothing but forms of

cerebral excitement) called *joy* and *grief*. It is also excited by irritating impressions on the eye-balls and eye-lids—and in children by pain in any part of the body—in each case the lachrymal gland being at a distance from the stimulated point. Were the primary irritation made on the gland itself, there is no reason to believe that the result would be a secretion of tears.

2. *Secretion of saliva.* This is excited by impressions, whether agreeable or disagreeable, on the tongue, lips, and palate—as well as by the sight and odor of agreeable food, and the remembrance and conception of savory substances. Yet in neither case is the salivary gland proximately excited. The excitement it experiences is exclusively sympathetic.

3. *Perspiration or sweat.* This cutaneous discharge is excited by an impression on the stomach, produced by liquids either cold or hot. When a person heated by hot weather or severe exercise swallows hastily a draught of cold water, his skin secretes promptly an abundance of sweat. On the same principle of sympathy an immersion of the feet and ancles in warm water often excites perspiration over the whole body.

4. *Secretion of bile.* This secretion is excited through sympathy by an impression made on the mucous lining of the stomach and intestines, by calomel, aloes, and sundry other medicinal agents. Nor have we the slightest reason to believe, that an impression made immediately on the liver, by sprinkling the same articles over its surface, or injecting them into its blood vessels, would be equally effective in the production of bile. On the contrary, hepatitis, accompanied by a suppression of the biliary secretion, would much more probably be the result of such an experiment.

5. *Secretion of urine.* Of this secretion the same may be said. It is best promoted by remote stimulation. An impression made on the stomach by ripe water-melons produces frequently, and in a short time, a copious flow of liquid from the kidneys. The same effect is often induced by a draught of cold water alone—by water impregnated with some vegeta-

ble acid, or by the application of cold water to the surface of the body.

Of the secretion of semen masculinum we forbear to speak in detail, else could we easily show, that it also is essentially connected with remote stimulation. And of the pancreatic, the serous, and the mucous secretions, it would be easy to make it appear, that the same is true.

Exemplifications of formative or constructive action show themselves abundantly in the general *nutrition* of the body, and in all sorts of tumors, and other forms of unnatural growth.

The foregoing are the principal kinds of secretive and formative action in the body. And that they are all promoted, and may be greatly augmented, by remote stimulation, no physiologist will venture to deny. Nor can he refuse to acknowledge that to proximate or immediate stimulation their production is not necessarily, nor even usually attributable. And we fearlessly repeat, that were the stimulating agents, which give rise to the sympathetic action, applied to the secreting organs themselves, we are not authorized to believe that the healthy functions of those organs would be in any measure increased. The reverse, we doubt not, would certainly occur.

But why should we dwell any longer on this subject? The principle for which we are contending is a general one. Every vital action, of whatever form or description, we repeat again, is the result of stimulation. And a vast majority of these actions are of necessity the product of remote stimulation. Indeed the effects of this sort of stimulation are seen in all parts of the body; those of proximate or immediate in comparatively but few parts. And this is true, whether the system be in health or disease, as might be easily shown, had we time and room to prepare and exhibit an analysis of the subject. In the course of such analysis we could make it clearly appear, that the parts of the body are few in which it is even possible for proximate stimulation to occur.

Such are the remarks which we have deemed it necessary

to premise, that we may be the more easily and clearly understood, in the exhibition we are about to offer of the sympathetic theory of impregnation. And we trust they will prove sufficient for the purpose intended. As further preparatory however to their application, we ask the reader to bear in mind the well known and important fact, to which allusion has been already made, that every organ or part in the generative system of woman sympathizes very strongly with every other part. The commencement and course of the process of impregnation appear to be as follows.

I. The semen masculinum being deposited in the vagina, produces there its specific impression, which can alone be productive of the specific effect intended to be the second step in the process.

II. With this vaginal impression the ovarium next sympathizes, taking on a specific and corresponding mode of action. And that mode is such as, by its secretory and formative character, to give the complete and finishing touch to the ovum—a principal element of which is the "germinal vesicle."

III. The third form of action that shows itself sympathetically, is in the fallopian tubes, which by becoming rigid, and more highly charged with arterial blood, and of course with vital energy, and undergoing other changes in size and figure, pass into a state of preparation to receive the ovum from the ovary, and convey it into the uterus.

IV. Next occur, by sympathy, in the fundus uteri, the changes designed to fit it for the reception, attachment, and general accommodation of the ovum, by the formation of the maternal portion of the membranes, and the placenta. Nor is this all.

On the same principle, and in promotion of the same object, a change occurs in the action of the membrane which lines the neck of the uterus, the design of which is, the obstruction of the canal leading into the vagina, to prevent the escape of the ovum in that direction.

Nor do the changes connected with impregnation end here. Far from it. The entire female system undergoes a striking

change. The mammæ enlarge and soften, the nervous system becomes more sensitive and excitable, uncommon tastes and longings, sentiments and propensities often occur, and, toward the close of gestation, strongly-marked and important changes occur in the external organs of generation. These last changes are preparatory to parturition, which, without them could never be effected. But we must proceed no farther in detail on the subject.

The foregoing, though not all, are some of the leading changes, which, in consequence of impregnation, occur in the genital organs, and in other parts of the system of woman. And it is not easy to decide which of them is the most strikingly interesting and important. Yet the act of *immediate impregnation* excepted, they are all acknowledged to be the offspring of sympathy.

In truth to deny this, or even to question it, would be eminently preposterous. It would involve the weak and extraordinary notion, that the semen masculinum by its *actual presence in every altered part of the body, performs the whole work.* That it stimulates the fallopian tubes, increases their size, changes their shape, expands their fimbriæ, and renders them rigid. That, by its own direct and immediate action, it stimulates the fundus uteri into the preparation it makes to receive, and retain, and accommodate the ovum. That, on the same ground, and by its own immediate influence, it induces the mucous lining of the neck of the uterus to act in such a way as to close the mouth of that organ, the more certainly to prevent the loose ovum from escaping into the vagina. And that it extends in substance to the mammæ, and produces in them the changes they undergo.

All these changes we say must be produced by either the actual presence of the semen masculinum *in*, and its action immediately *on* the organs changed, or by *sympathy* with the primitive change produced by it, as most other persons allege, in the ovarium, but, as we believe, in the vagina, where *we know* that it is deposited. That any physiologist, we repeat, can even sportively imagine, much less seriously con-

tend for, the former of these modes of the production of such changes, we believe to be impossible. Some form of the latter therefore must necessarily be adopted, or the subject dismissed, as altogether inscrutable.

If then sympathy produces all the other changes which occur after impregnation, in various parts of the female system —in the fallopian tubes, the fundus uteri, the body and neck of the uterus, the mammæ, the external organs of generation, and elsewhere—if sympathy does all this, may it not also produce (what would seem to be much more easily done) that change, denominated impregnation, which occurs in a small portion of the ovarium? A reply to this question in the affirmative is but a simple inference from the *major* to the *minor* —a conclusion that, if sympathy can accomplish the former achievement, great and complicated as it is, *a fortiori* it can accomplish the latter.

We defy the ablest and most ingenious opponent of the sympathetic doctrine to render even a specious reason why, if for the production of impregnation, the *presence* of semen masculinum on the ovarium be *indispensable*, it is not equally so at the mammæ, and other parts of the female system, to produce in them the changes which follow. Nor will any one, on fair terms, accept the challenge, from a full conviction that the day will go against him. We, on the contrary, hold ourselves prepared to submit to our readers as plausible and sound reasons why semen masculinum should be present in the mammæ, to produce in them the secretion of milk, and the other changes in action and condition which they undergo during pregnancy, as the ablest humoralist can give, why medicinally substances of any description must be *formally* mingled with the blood, in order to prove either beneficial or prejudicial to deranged organs.

At the risk of being deemed unnecessarily wordy, we again express our regret, that a want of room prevents us from republishing Dr. Haighton's account of his experiments in defence of the sympathetic theory of impregnation. As far as we are informed, these experiments have never received from

any quarter, a reply of force sufficient to shake them in the slightest degree; and, as already intimated, they are, in our own opinion, conclusive of the controversy.

Were it admissible for us to protract this discussion any farther, we could also draw from the vegetable, and the lowest orders of the animal kingdom, sundry analogies highly favorable to the same theory. But we content ourselves, for the present, with a brief statement of the following curious and interesting fact. And for its authentiaity we refer to writers on entomology.

The aphis (cabbage-fly, or rather cabbage louse) produces in the course of the year several generations of descendants—one of them *oviparous*, and all the others *viviparous*.

The oviparous is hatched in the spring—say in April or May. This is followed successiyely by the viviparous, throughout the summer and early autumn, when, at a more advanced period, another generation, or deposition of eggs occurs, which lies dormant during the winter, and is hatched again in the following spring. And thus, from year to year, goes on their reproduction. But the most striking and extraordinary feature of the process remains to be mentioned, and is as follows.

The first spring-brood (that we mean which is of oviparous birth) consists of males and females. These unite and celebrate their loves, like other insects, and the males immediately die.

The next generation is viviparous, and all are females. This generation, without sexual intercourse (the males being all dead) gives birth to another generation, which, in like manner, consists also entirely of females. This race produces a a third, still viviparous, and still of the same sex. And thus viviparous reproduction proceeds, until some time in the autumn, when another crop of eggs is deposited to be awakened into active life, and moulded into due form, by the returning spring. Such are the facts of the case. Now for the philosophy.

Here are several successive generations (we think from five to seven) where the female has become pregnant, and given

birth to offspring, without copulation. Whence, we ask, is derived the male semen, to come into contact with the ovarium, as an indispensable event, and produce impregnation? To the advocates of the contact-hypothesis the question is addressed, for such solution as may await it, from their *wisdom* and *philosophy.*

We ourselves very frankly confess, that we have neither the one nor the other adapted to the subject. To us the event is therefore inexplicable; yet not more so than every other connected with generation. For the entire process is inexplicable. And in real inexplicability there are no degrees. The whole achievement is a dark mass. We understand no better how impregnation is effected with copulation, than without it. The impregnation of the female aphis after the death of the male, and therefore *without* his immediate influence, we understand just as well, as we do the impregnation of any other female animal *with* the influence of the male, and during his lifetime. Nor is there, as we confidently believe, any more necessity, as regards impregnation, for the semen masculinum to come into contact with the ovarium in woman, nor any more truth in the notion that it does so, than there is in the case of the female aphis, in those impregnations which occur in her, after the death of all the males of her race.

In no other case, except that of the female aphis, do we contend for the existence of *conclusive* evidence that impregnation can be and is effected, without the *possibility* of contact taking place between the male seminal fluid, and the ovarium. But in that case the impossibility is complete.

Analogy therefore, amounting to strong probability, which, under a deficiency of positive evidence, has great weight, forcibly indicates that a similar condition of things may prevail in the human subject—similar, we mean, as to the actual impossibility of contact occurring between the semen masculinum and the ovarium. And our reasons for believing that it positively does prevail, have been already submitted to the consideration of the reader. A few thoughts on the probable purposes and uses of *menstruation* shall close this article.

Though we believe it to be, with physiologists, the prevailing opinion, that the function of menstruation is peculiar to woman (the females of all the inferior animals being strictly *amenstruous,*) yet it is asserted by naturalists not unworthy of credit, whose opportunities to become acquainted with the matter in question have been favorable, that the case is otherwise; and that the females of the ape tribes are menstruant.

Another attribute deemed peculiar to woman is, that she admits, at all times the sexual embrace, the females of the inferior animals admitting it only periodically, and generally after long periods of intervening abstinence. But to this usage also the female ape is represented as an exception. She is said to receive the male, if not at *all* times, at least at much shorter intervals, than the females of other tribes of the inferior animals. Of these facts (for the truth of which however we do not fully and definitively vouch) the legitimate bearing will presently appear. But previously to this a remark or two on the nature of the menstrual function may not be amiss. And the object at which we immediately aim, at this moment, is sufficiently embraced in a correct reply to the following question.

Whether is menstruation a hemorrhagic, or a secretory function? in other and plainer words, whether is the menstrual fluid real blood, or something prepared from the blood, by a secretory process? To this interrogatory our reply is brief. We deem menstruation a secretory function, for the following reasons.

1. The menstrual fluid is not blood. Why? Because it does not, like blood, coagulate; nor does it, of course, contain any true fibrin, the master element, and the true cause of the coagulation of blood. It consists of only red globules, gelatine, and serum, which, of themselves do not, we repeat, constitute blood—wanting, as they do, the most important element in the composition of that fluid. In cases of real uterine hemorrhagy, the fluid discharged does coagulate, contains fibrin, and is therefore genuine blood. In evidence of its true coagulation,

it is discharged in clots. But the menstrual discharge, in a natural and healthy condition, is always fluid.

2. The odor of the menstrual fluid is peculiar—we might say specific—and altogether different from that of blood. It is truly as *sui generis*, as the odor of assafœtida, castor, or urine.

3. The menstrual is a *dead* liquid, blood a *living* one. When chemically analyzed, the former affords results quite different from those of the latter, under similar treatment.

4. Like other glandular bodies, the uterus receives more blood than is requisite for its nourishment. Hence like such bodies it frees itself from the superfluous portion of that fluid, not by a hemorrhage, but a secretory process. All natural and healthy discharges of fluids from the human body are the results of secretion.

Another question respecting menstruation is frequently asked, and differently answered by different physiologists. Whether is it a healthy and natural, or a morbid and unnatural process? Answer. The former certainly. It is as necessary to the health of woman, and therefore itself as healthful a process, as is that of digestion or the secretion of bile. Amenstruous women are never healthy. Is it possible then for deranged action in an important and influential organ to contribute to the sound condition of the whole body? An affirmative reply to this question from the lips or the pen of an enlightened physiologist would greatly surprise us. We ourselves reply, that we deem an issue of the kind physically *impossible;* because it is contrary to all we know of the laws and economy of nature. To contend or even admit, that an unsound *part* can tend to or effectuate the production of a sound *whole*, would amount to as positive an absurdity, as any other self-contradiction that can be uttered or conceived.

True; amenstruous women do occasionally bear children. But that avails not. They rarely if ever bear healthy children; and are *never* healthy during gestation. All observation compels us to believe that menstruation is essential to the perfectly sound and efficient condition of the generative or-

gans and their functions in woman. In fact the notion that the healthy propagation of our race depends on morbid action in the propagating organs is so utterly preposterous, as to be unworthy of a serious effort to refute it. It is so self contradictory as to set nature at war with herself for the purpose of insuring a salutary result—of actually *bringing physical good out of physical evil.* As reasonably and well might the advocates of this hypothesis contend, that some morbid affection of his genital organs renders man also more competent to the propagation of his race, than he would be without it. Indeed reasons not easily refuted might be given, why, toward the production of a sound and vigorous offspring, the health and good condition of women should contribute even more than those of men. Corroborative, and even confirmatory of this, if we mistake not, is the result of observation in all enlightened nations, where the subject has received the attention it deserves. In Turkey, Persia, and Arabia, the belief in the doctrine here suggested is universal among the more intelligent and observing classes of society. The Arabs apply it in a particular manner to their domestic animals. Hence young and sound Arab mares of the *first order* are rarely if ever sold, or given as presents. Not one has ever been imported into America; and few if any into Europe. Males on the contrary are habitually sold and exported as presents.

As respects the design and usefulness of menstruation, some light may probably be thrown on them by a comparison of the process with an analogous event which occurs in the females of many sorts, perhaps indeed of all sorts of inferior animals of the higher orders. We refer to what is called the *rut* or *heat*, a condition of the genital organs which appears to be designed for two purposes—to awaken and vivify the sexual appetite in the female, so that she may freely admit the male; and to prepare her organs for the work of impregnation. We consider these to be the intention and use of the rut in the females of the inferior animals, because it is during the period of the existence of that state of the parts, and no other, that they admit the male to sexual connexion with them, or are susceptible of impregnation, were they to do so. And, as already

mentioned, the rut in the females of inferior animals is strongly analogous to menstruation in woman. So similar indeed are the two conditions to each other, that the mere *discharge* of the menstrual fluid constitutes their only appreciable difference.

The rut therefore, we say, is and does to the females of the inferior animals what menstruation is and does to human females. It enlivens and heightens in them the sexual appetite, and prepares them the better for the process of impregnation. A few details on this topic will demonstrate, if we mistake not, the truth of our position. Some of these details we derive from the effects or accompaniments of the rut in the females of our domestic animals. And they are strongly analogous to the phenomena of menstruation.

During the time of menstruation, it is well known, from ocular inspection, that the ovaries and whole generative system of woman are unusually engorged with arterial blood. So is the generative system of the cow, the mare, the ewe, and all other female animals, during the time of their rut or heat. And, during the same period, though the external organs of our domestic female animals do not discharge a genuine menstrual fluid, in the usual acceptation of the term, they discharge something strongly resembling it. The fluid is neither blood-like, nor large in quantity; but it is very perceptible to the eye, frequently witnessed, and not unfrequently tinged with blood.

Thus analogous are the phenomena of menstruation and rut. And in their effects on the female generative system, as regards its susceptibility of impregnation, they are also similar. As already stated, the females of our domestic animals, as well as of the deer, the rabbit, the hare, and others both wild and tame, are impregnated only, and admit the male only, during their heat, or rutting season. And, though woman admits the sexual embrace at all seasons, she does so with more warmth and strength of feeling just before and after the period of menstruation, than at any other. And she is then, in like manner, most susceptible of impregnation. Hence married women, whose sexual intercourse is uninter-

rupted, begin habitually their count of pregnancy from about the close of a menstrual period. And their calculation is rarely incorrect.

But wherefore does woman permit and enjoy the sexual embrace, at all times—other female animals doing so periodically, and only at long intervals? The answer is deemed obvious and easy. *Because she frequently menstruates.* On each occasion her genital system, especially the ovaries and uterus, becomes abundantly injected with arterial blood—an event which, whenever or wherever it *healthily* takes place, always refreshes the part, invigorates its vital properties, and fits it for a more energetic and efficient performance of its functions. And the intervals between the menstrual periods are so brief, that the condition of augmented feeling and vigor imparted to the genital organs, at a preceding period, is not all lost, before the occurrence of a subsequent one. Hence the constant sexual feeling of woman, and the permanency of her fitness for impregnation—marked, however, as already stated, by different degrees of these attributes, at different times.

Although we now, for the first time, publish these views on the nature, effects, and uses of menstruation, yet have we entertained and defended them, both publicly and privately, for many years. Nor, as far as our knowledge and recollection serve us, have they been published or entertained by any body else.

We claim them, therefore, as our own, assume the responsibility of any errors they may contain, and deem ourselves entitled to such credit and consideration as they may be thought calculated to confer. Their correctness receives additional confirmation from the two following facts. In woman the sexual appetite greatly declines—is at times entirely extinguished, and she ceases to be fruitful, as soon as her catamenial function disappears. Without any farther illustration or proof, therefore, we submit them to the unprejudiced judgment of the reader, not without some hope, that he will not find them altogether unworthy of his attention, nor peradventure of his approval.

C. C.

Selections from American and Foreign Journals.

Case of enormously enlarged Stomach. By John Fondey, M. D.—Professor Horner has handed us the following history of a patient, whose stomach has recently been deposited in the Museum of the University of Pennsylvania. It is of enormous size, probably without a parallel.

Lindsay S. S., aged about twenty-three, was originally from Kentucky (his native place I know not), but for some years past he resided in Tazewell County, Illinois. He was rather a wild youth, and became dissipated in his habits, drinking freely of ardent spirits, yet, although he was in the habit of indulging in very large potations, he was never known to stagger, or indicate in any manner, that he was under the influence of stimulus. In the course of a few years he became the subject of the enormous appetite which constituted so prominent a feature in the history of his disease. Soon after (the exact time I know not), he became insane, yet having lucid intervals, during which he was capable of managing his affairs, and fulfilling his civil and social relations. His insanity was of a harmless character, a prominent feature of which consisted in a propensity to appropriate to himself little articles, no matter how trifling, and he seemed to take as much delight in securing an old nail, could he only do it unobserved, as others would in appropriating to themselves something more valuable, and fitted to excite human cupidity.

The amount of food consumed by him at a meal was very large. He would eat at one meal at least two gallons of mush and milk, and other things in proportion. Sixteen large cups of coffee were about the smallest quantity of this beverage taken by him. Usually this large amount of food was thrown off by his stomach, though sometimes it was retained. He would eat several meals a day, four or five in number, all of them as large as the above in quantity.

By some physicians he was supposed to have a tape worm, by others his disease was attributed to an irritation, or subacute inflammation of the mucous membrane, or membranes of the stomach; the correctness of which diagnosis was fully shown on dissection. During his periods of insanity he was strongly inclined to wander, and was placed in the Belleville prison, in consequence of some little theft committed by him in his paroxysm. He was, therefore, kept in some degree of confinement at the house of his brother, from which he managed to escape, and came as far as Canton, near which he received the lynching which resulted in his death.

His disease had by this time produced some effect upon his frame—he was emaciated, though naturally of a good constitution. He travelled in company with an individual of his acquaintance, and when they arrived at Canton he took several keys out of the doors of different shop keepers and put them in his pocket. After they had gone a few miles from Canton, two men overtook them, and charged him with having stolen a trunk from the hotel—he denied it, but confessed that he had taken the keys, and restored them. His companion, however, who was, undoubtedly, the real thief, then affirmed that Scott *had* stolen the trunk, and said there was no use in taking him to Peoria for trial, for they knew him in that place to be a fool, and would only let him go free—that while they had him in their power they had better take the punishment in their own hands. Hereupon they took a teamster's whip, with a long thick silk lash to it, and each in turn whipped him on the back, until the lash was wholly worn out, and then cutting a thick hickory stick, they renewed their blows until they had given him about 250 lashes. He was then released. This was on Monday, December 17th, 1838, and on Sunday evening he arrived at Columbus, Adams County, the place in which I was then located. He had, I understood from some passengers who came in the same stage, eaten three or four enormous meals that day, and seemed rather wild—at times dozing, and complaining much of being half dead with hysterics. His gait was rather unsteady, and he seemed extremely weak. On entering the hotel he requested some water; after it was brought, he laid down, and in ten minutes an individual found him apparently asleep. Some time after, however, on attempting to arouse him, no answer was returned, nor could he be awakened. I was then sent for. When I saw him I found him perfectly destitute of sense or motion. Not a muscle (except

those of respiration) moved; his breathing easy, without stertor; pulse variable—now full and hard, then weak, feeble and intermittent; temperature also changeable—at one moment his face flushed, at the next pale—his legs at one moment warm, very soon cold, and then warm again. On flexing the joints, the muscles did not keep the bones in the position thus given them, as in catalepsy. All his symptoms seemed to indicate a prostration of nervous energy, and remedies were prescribed suited to such a condition of the system. He seemed at times dead, but would come to again—this continued on—all remedies were unavailing, and on Monday morning, Dec. 24th, 1838, at 2 o'clock, about eight hours after his arrival at Columbus, he died. After death, however, his eyes remained so clear and prominent, and his skin so elastic and warm, that I doubted whether he was dead, and employed means to bring him to, but on the second day he gave evidence that the principle of life had departed, and he was consigned to the grave.

Four weeks after, his brother arrived, for the purpose of having him examined, preparatory to the trial of those who had beaten him, and I was requested to conduct the dissection. The weather was cold and windy, and we were obliged to examine him in the open air; besides that, we had so little time for examination, that I was not able to make as thorough and satifactory an investigation as I could have wished—in fact I was compelled to leave some organs unexamined, and those which were examined it was found necessary to hurry over, so that if the account of the post mortem appearances is not as full as you would desire, you will, I trust, attribute it to the circumstances in which I was placed.

The skin indicated the reception of a great many lashes, some of them superficial in their effects, others extending down to the bone, as indicated by the immobility of the skin at these points, its hardness, and apparent attachment to the bones. Extensive sugillation was also apparent on the dorsal aspect of the trunk, especially in the lumbar region and the inferior portion of the dorsal. On tearing away the calvarium, the dura mater was found to adhere to it with great tenacity—the parietal foramen rather large, and considerable blood, grumous, yet somewhat fluid, issued from the parietal vein. The vessels of the membranes of the brain, especially those of the pia mater, were rather full of blood—not more congestion, however, I should imagine, than we should look for in one who had labored for some years under insanity. The substance of the brain was very soft, whether from in-

cipient putrefaction, or from the disease, I know not, most probably from the first cause, as no red points could be discovered on making incisions in its substance. Nothing unusual was discovered in the spine, though the examination was here hurried over, from an apprehension that sufficient time would not be left for the investigation of the other organs.

On opening the abdomen, the small intestines were found to be nearly equal in size to the colon. A large portion of them was slit in order to ascertain whether any tape worm was present or not. Nothing of the kind was, however, visible—a little mush seemed to be all that occupied the intestines. No conveniences were at hand to wash the bowel and ascertain the appearance of its internal surface. The spleen and liver were enlarged; want of time prevented an examination of them. There seemed to be a total absence of fat in the parts which were examined. The thoracic cavity was not opened, by reason of the shortness of time.

As regards the appearance of the stomach, you, sir, are better qualified than myself to ascertain the changes of structure it has undergone. I will not, therefore, say any thing on this subject, but merely observe, that it was much more rubescent at the time of the dissection, than when I handed it to you.—*Med. Examiner.*

Professional Incomes in London.—An interesting Memoir of the late Dr. James Hope, the distinguished author of the Treatise on Diseases of the Heart, has been recently published by Mrs. Hope. The following particulars, in relation to the incomes of the first grade of practitioners in London, will be read with interest:—

On arriving in London, Dr. Hope was led into the belief that the first twenty physicians of the metropolis divided about £80,000 annually between them, and that a successful physician might hope to be established in good practice in about five years. To be one of so large a number as twenty seemed no difficult task, and therefore "he ignorantly hoped" that if he succeeded at all, he should be making £4000 a year in about five years! He was, of course, quickly undeceived. He had now gained many friends, he was married, he was a successul author, but the approaches to £4000 a year were "very tardy." He thought himself in fault, until a little friendly conversation with Dr. Chambers and Sir Henry Halford taught him more correctly upon the subject of so speedily

stepping into large receipts. Dr. Hope kept a regular account of every fee which he received during the twelve years he was in London. "We are, therefore," says the Editor, "enabled to speak with the greatest accuracy." The first two years he was in London he made £200 a year. The third year, the accidental removal of some families who employed him, reduced his practice to £150. At the end of the third year his work on the Diseases of the Heart was published, and he came before the profession as a physician to the Marylebone Infirmary. Still, in the first year after, his practice was little increased; but from this period his reputation became extended, and his practice gradually increased, until in eight years more, when he retired, he was making £4000 a year. This—as we should call it—great success was fairly and honorably labored for and won.—*Med. Examiner*, from *Lond. Med. Gaz.*

Bourgery on the Anatomy of the Spleen.—M. Bourgery considers the spleen to consist of ten component parts:—1. Vesicular membranes; 2. blood-vessels; 3. floating vascular corpuscles; 4. a granulo-capillary ground; 5. a splenic liquid; 6. splenic glands; 7. lymphatic vessels; 8. nerves; 9. cellular tissue; 10. the enveloping membrane of the spleen.

The splenic vesicles are uniformly spread over the whole extent of the organ, are of an irregular polyhedral, and, when fully injected, of a spheroidal or oval shape, varying in size in different animals, and also in the same spleen. In man their mean diameter is from 1 to 1¼ millimetre. Each vesicle does not form a simple cavity, its walls being traversed by vessels which form projections in the interior, covered by the lining membrane. In consequence of this arrangement the cavity is divided into cells, at the bottom of which the minute glands and capillary vessels are seen in relief. There are orifices in vesicles of two sorts:—1. Orifices of communication more or less irregularly circular, whose edges are thin, and formed by a fold of the membrane which lines the cavity. Their diameter is from a quarter to half of that of the vesicle. There are one, two, or three orifices in each vesicle; and to this reciprocal communication is due the ready inflation of the organ, not only from the veins, but also from an aperture on any part of its surface. 2. The venous orifices, less numerous than the communicating, and scattered here and there, a single vesicle sometimes containing two or three, at others one cannot

be found in a group of several vesicles. They are of a circular or elliptical shape, one-twelfth of a millimetre in diameter, and are the absorbing mouths of veins. Between the vesicles are septa, or intervesicular spaces, formed by the separation of their lining membrane, and containing the splenic glands and vessels, their size varying with the greater or less repletion of the organ, but generally bearing the relation of 2 to 3 to the vesicular part. The investing membrane, forming the walls of the cells, is continuous through the whole extent of the spleen, and may be considered as one homogeneous whole, divided into numerous little ampullæ. It is supported by vessels and glands, and forms a sort of frame-work to the organ. This membrane is very complicated in its structure, and cannot therefore be considered, as by Malpighi, to be a simple dilatation of the internal tunic of the veins.

The splenic arteries and veins, running side by side, are directed towards the periphery of the spleen, the veins being pierced in their whole extent by little circular apertures, leading into the smaller intervesicular branches, which are distributed to the splenic glands and the membranes of the vesicles. The small vessels project into the cavities of the vesicles in a peculiar manner to reach the floating corpuscules, and present the appearance of a bunch of grapes. Lastly, all the small vessels of the spleen, in the turgid state, present numerous dilatations and contractions, so as to give them a marked knotted appearance.

The corpuscules are small bodies floating in the cavities of the vesicles, to the walls of which they are attached in a pediculated manner, by the extreme branches of the blood-vessels and lymphatics. They are about fourteen or fifteen times the size of the blood-globules. By the granulo-capillary basis is intended to be represented the two structures placed beneath the vesicular membrane, viz. spherical pale granules, or glands, four or five times the diameter of the blood-globules, and the capillary net-work of veins, arteries, and lymphatics.

The splenic liquid, which appears to be elaborated by the floating corpuscules and the granulo-capillary basis, is deposited in the cavities of the vesicles, or taken up by the absorbing veins or their walls. It is thick, viscous, of a reddish-brown color, and under the microscope appears to consist of globules suspended in a yellowish unctuous fluid. 1. Lenticular globules, some of which are surrounded with a red border, and appear to differ very little from ordinary blood-globules; others being colorless. 2. Whitish globules, irregular in form and size, and resembling those found in the chyle and lymph.

The splenic glands, so far as actual volume and consistence are concerned, form the principal organic element of the spleen. They fill, with the ramifications of the vessels, the intervesicular spaces. Their diameter is a quarter of a milli metre. They are united by bands of a similar nature with themselves, which extend over every part of the spleen. From the afferent and efferent lymphatic trunks, and the dispositions of the vessels about them, M. Bourgery is convinced that they are simply minute lymphatic glands. The lymphatic vessels are extremely numerous in the spleen, and in many situations remarkable in their structure, being enlarged at intervals, and, in addition to their valves, divided in their interior by septa, which gives them a sort of rudimentary glandular structure, and shows them to be not merely canals for transport, but also, in some degree, organs of elaboration.

M. Bourgery considers the spleen as an apparatus for the elaboration of the blood, and draws an analogy between it and the lymphatic glands; observing that if, in relation to its anatomical structure, we may define the spleen as a vast lymphatico-sanguineous gland, on the other hand, lymphatic glands generally may be considered, to a certain extent, as small spleens appended to different parts of the circulatory apparatus. In treating of the intimate structure of these glands, we shall see how far the idea of the conformity of these two sorts of organs, evident so far as the glandular structure of the spleen is concerned, is borne out by the internal organization of the canals of the lymphatic glands.—*Ibid, from Gazette Médicale.*

Ligature of the External Iliac Artery for Femoral Aneurism. By Wm. Power, M. D., one of the Attending Physicians to the Baltimore Alms-house Hospital.—Christopher Daly, a native of Baltimore County, æt. 61, entered the surgical ward on the 1st of July; a man of stout frame, very youthful appearance for one of his years, and enjoying excellent health with the exception of the aneurismal disease for which he came to the hospital. For the last fourteen years he has enjoyed good health, uninterrupted by an attack of any sort unfitting him for his daily labor. About fifteen months ago, according to his best recollection, his attention was first accidentally called to the aneurismal tumor whilst digging in a ditch, and standing in water, though with his feet well protected. Placing his fingers upon it, he says he "distinctly felt

a pulse, but thought it was in a very strange place."—As it gave him no pain or inconvenience he thought it was nothing serious and continued to work on as usual. About a month had elapsed from this time, when he was suddenly seized with violent cramps in the diseased limb, and finding exertion painful, he stopped his usual employment, but assisted during the following harvest in the lighter kind of field labor. The limb was slightly swollen below the tumor when he first discovered it; but after the attacks of cramp, which were from time to time repeated, the swelling increased, together with a sense of numbness diffusing itself over the whole leg. The tumor has steadily continued to increase, together with the œdema of the leg. He has, for the last eleven months, kept quiet and strictly avoided all exertion, and has undergone no treatment until he came under our care. The aneurismal tumor was situated in the right groin immediately below Poupart's ligament, which was forced a little upwards in an arched form. It was of an irregularly rounded form, about three and a half inches in its perpendicular, and three in its transverse diameter—pointing a little upwards and inwards, where the pulsation was more distinctly seen and felt than elsewhere. Its pulsation was evident to the eye—communicated a distinct thrill to the hand when laid upon it, a *bruit de rape* to the stethoscope, and the pulsations were eccentric. Compression upon the iliac artery, on the psoæ muscles, arrested all pulsation in the tumor. The external iliac for an inch above the tumor felt larger than the artery of the opposite side, and both here and in the continuation of the femoral below the tumor, gave a distinct file-sound to the stethoscope. The lymphatic glands of the groin were swollen and hard—the leg was one-third larger than that of the opposite side—purplish in hue, one degree higher in temperature, affected frequently with cramps, and pain with a sense of numbness over the knee and instep.

Tongue clean, appetite good, bowels regular, skin pleasant, pulse 80—natural, sounds of the heart clear and distinct, chest every where resonant, respiratory murmur vesicular, no ossification of the arteries at the wrist. The rest and quiet of the hospital reduced in a degree the œdema of the leg, and caused the tumor to appear more prominent. He was placed on mild diet, and one or two gentle aperients administered. His health remained undisturbed, and his spirits good up to the 19th of July, when, assisted by my colleague, Dr. Annan, and in the presence of several medical friends, the following operation was performed.

An enema having been previously administered, and gutt. lx. Tr. opii, given an hour previously, the patient was placed upon upon a suitable table, his pelvis inclining towards the left side, and an incision made through the skin and common integuments of three inches and a half in length, extending from three-fourths of an inch to the inside, and on a level with the antero-superior spinous process of the ilium, with a curve presenting downwards to about three-fourths of an inch above the external ring—a small vessel running to the skin was divided in the inner angle of this wound and secured. The glistening fibres of the fascia of the external oblique were then divided, according to a suggestion in the last edition of Dr. N. R. Smith's work on the arteries, in the same direction, but a half inch above Poupart's ligament. I now attempted to pass my finger under the edge of the external oblique and fascia transversalis, but found the border of the fascia of the external oblique presented such obstruction to free manipulation, by its resistance, that, to facilitate my farther progress, I was obliged to divide it with a buttoned bistoury, thus making a T incision in this fascia. The finger now passed readily down on the outer side of the internal ring, avoiding the cord, and after a little dissection with the nail, came in contact with the artery. Fearing, however, the soundness of the coats of the vessel thus low down, the sac of the peritoneum was gently pushed upwards and separated from the fascia transversalis for an inch and a half; and this membrane, together with the fibres of the internal oblique and transversalis divided by a buttoned bistoury on the finger for three-fourths of an inch. This sufficiently enlarged the wound for easy manipulation. The finger sought the artery about three inches from Poupart's ligament, and with one of Prof. Gibson's needles, armed with a ligature of two strands of sadler's silk, waxed but not twisted, an attempt was made to pass it from without inwards; finding the curve of the needle so great that I could not depress the handle sufficiently on the abdomen, I cautiously separated with the nail the vein from the artery, and passed it from within outwards. The ligature was then drawn tight with some difficulty at this depth, my assistant pressing on the knot until the second was tied. The pulsation immediately ceased in the tumor. The wound was brought together by two points of suture and a few adhesive straps. The operation lasted 25 minutes, and was borne extremely well by the patient. The patient, after the wound was dressed, was carried into a well-ventilated room, and placed on a bed with the limb of the side operated upon well flexed on the pelvis.

Complains only of fatigue and numbness of the right limb, with dull pain of the loins and hypogastric region—pulse 80—natural—at 7, P. M. Temperature of right foot, as tested by the thermometer, seven degrees lower than that of the opposite side—gave Tr. opii, gutt. xxx.

July 20th. Patient rested tolerably during the night—slept nearly four hours; countenance cheerful; some nausea and anorexia. Pulse 80, as yesterday; skin pleasant; complains of dull pain in the loins, and some cramps in the leg. Tumor decreased one-third in size; hard on its external side; slight pulsation at its inner and upper portion. Right leg florid, only two degrees lower in temperature than the left; ordered venesec. 14 oz. ℞. Morphiæ acet. gr. ij.; aquæ camphor. ℥iv.—dessert-spoonful every two hours. Towards evening his pulse rose to 114. Takes barley water made cold with ice.

21st. Appearance much altered; countenance anxious, haggard; eyes sunken and suffused; voice weak; nausea, constant retching; complete anorexia. Tongue reddish along the edges; thirst; skin bathed in perspiration; pulse 140, small; abdomen tympanitic; complains of sharp pain along the recti muscles, in the iliac region and about the loins; bladder irritable, trying to make water every ten minutes; venesec. oz. 14. ℞. Calomel grs. xv., pulv. opii, grs. ij. Added two drops of hydrocyanic acid to each dose of the solution of morphine; forty good leeches applied to the abdomen. At 7 P. M. the leech-bites were still running freely, and had afforded great relief; pulse 130; blood drawn this morning well buffed; still restless. Continue anodyne. Tumor as yesterday. Temperature equal on both sides.

22d. Countenance much improved; pain and uneasiness much less; abdomen still tympanitic; no movement of the bowels. Pulse 125, fuller but soft; tongue moist; thirst less, and asks for food. Still complains of pain in the back and abdomen; bladder still irritable; venesec. 8. oz. ℞. Cal. and opium as yesterday: hop fomentation over abdomen. 7. P. M. Still improving; skin pleasant; demands food; no passage; blood drawn in the morning lightly buffed; the bleeding made a sensible impression upon his pulse. ℞. Cal. grs. viij.; opii, gr. j.; chicken water, and milk iced. Continue solution.

23d. Passed a good night, and makes no complaint this morning excepting of his bladder; abdomen much softer; skin pleasant. Pulse 96, soft; tongue clean; no stool; little thirst; wound gives no pain; an erythematous blush surrounding it for some inches; complains of the limb. ℞. Sal Rochelle,

℥j.; aqua, ℥viij.: wine-glassful every hour. 7 P. M. Bowels not moved; complains of flatus. Ordered a simple enema.

24th. Looks as well as before the operation; makes no complaint; pulse 90, soft; tongue clean. Peritonitis fairly subdued, and we looked forward to a happy result; waking from a sleep at one o'clock he turned over in bed; his nurse a few minutes after was struck by his respiration and altered facies, and a quantity of blood in the bed. I saw him a few moments after, pale, restless, pulse scarcely perceptible, almost unconscious, his bed floating in blood—between two and three quarts. The hemorrhage had ceased; a compress was laid over it; nor did it return; he sunk rapidly, and died about 7 P. M. It was not deemed advisable to make any effort to secure the vessel, as the slightest shock would have determined dissolution under our hands.

Autopsy eighteen hours after death. Weather very warm. Head and face purplish; features swollen; bloody serum exuding from eyes; slight greenness about the clavicles and abdomen; rigidity considerable; muscles large and firm. The lips of the wound were united, excepting for a half inch on either side of the ligature; this space occupied by a clot of blood; the points of suture had held firmly, and there was no disturbance of parts.

The abdomen was opened by incisions from the umbilicus to the pubis, and to the edge of the ribs. Their flaps turned down exposed to the peritoneum. There was no effusion of serum; no adhesions; but over the tract of the artery, extending upwards towards the kidneys, and down into the pelvis to where the peritoneum invests the bladder, this membrane, in a space equal to the size of the hand, was of a bluish livid tint, more wrinkled than elsewhere, as if lightly parboiled, presenting numerous whitish yellow points, which could be scraped off with the nail; some flakes as large as a half-dime. The membrane was firm; no adhesion with the intestine. The peritonitis had been local, and was entirely subdued. The adhesions of the lips of the wound were then torn open; the skin and cellular tissue were the only parts adherent. The blood confined in the wound had insinuated itself upwards for some distance between the fascia transversalis and the peritoneum, as well as between this membrane and the fascia iliaca; about four ounces of clotted blood were removed from the wound. The artery was then exposed, and found with the ligature around it; when pressed upon, blood came from an opening immediately under the knot. It was then carefully dissected out and removed, from Poupart's ligament to

the bifurcation of the aorta, and laid open, as well as the external iliac of the opposite side. They were equal in size; at least one-third of an inch in diameter; the inner coat of a creamy yellow hue; a little rough. The inner coat, where the ligature had been applied, on the posterior half of the artery, was wrinkled and crimped, so as to be smoothed out with some difficulty, but was not cut through, nor was there any effusion of lymph. On the anterior part was the opening, at least one-half the circumference of the vessel. The edges a little ragged, of a purplish hue, within a quarter of an inch of the opening; friable, and breaking easily under the nail. The outer coat was livid to the same extent; softened and more friable than the rest of the vessel; a soft clot below, but none above the ligature. The vein perfectly sound; no further separated from the artery than by the needle in passing the ligature; this was applied about an inch and a half below the bifurcation of the common iliac, and about the same distance from the origin of the epigastric. The tumor in the groin was next laid open and cleansed of its clots. It was about three inches in each diameter, the anterior wall formed by the fascia lata, its lateral and posterior walls by the muscles of the thigh; irregularly anfractuous. The profunda, which was given off very high up, immediately under Poupart's ligament, formed a portion of its posterior boundary. The artery, for an inch above where the tumor commenced, was filled with osseous concretions, grating under the scalpel, and pricking the fingers. The continuation of the superficial femoral where it emerged from the tumor, from the number and regularity of the circular atheromatous bands, looked more like the trachea than an artery, The aneurismal tumor was very thin and prominent towards the upper and inner part of the thigh. It contained between four and six ounces of soft black clotted blood, and as much firm fibrin arranged in layers penetrating into the anfractuosities of the tumor.

The arterial system was everywhere well developed; these vessels having more than their usual calibre. The heart was large and flabby, its walls thin, a little fatty; the valves all sound; aorta not dilated; other viscera of the chest and abdomen healthy. Head not examined.—*Maryland Med. and Surg. Journal.* September, 1842.

Counter-irritants.—The following formulæ have been communicated by Dr. Turnbull to the editors of the Pharmaceutical Transactions:—

Tinctura Capsici Concentrati.

℞. Capsici Baccarum, ℥iv.
Spiritus Vini Rect., ℥xij.

Macera per dies septem et cola. (It may also be made with advantage by displacement.)

This concentrated tincture is used as an external application, and is found to be a powerful rubefacient and counter-irritant, for which purpose the ordinary tincture of capsicum is not sufficiently potent.

Veratria, dissolved in this tincture, acquires increased activity; the capsicum apparently facilitating its absorption into the skin. Four grains of veratria, dissolved in an ounce of the concentrated tincture of capsicum, will be found as powerful in its effect as twelve or fifteen grains dissolved in alcohol.

Pulvis Aluminis et Capsici.

℞. Aluminis Sulphatis, partes tres.
Tinct. Capsici concentrati, partem unam.
Misce et sicca.

A very small quantity of this powder, applied to the tonsils, is found more efficacious, in some cases, than an alum and capsicum gargle.

Unguentum Ipecacuanhæ.

℞. Pulveris Ipecacuanhæ, ℥ij.
Olei Olivæ, ℥ij.
Adipis, ℥ss.
M. ft. unguentum.

Unguentum Emetinæ.

℞. Emetinæ, *g.* xv.
Sp. Vini. Rect. q. s.
Adipis, ℥ss.
M. ft. unguentum.

Dr. Turnbull states, that he has found this ointment particularly efficacious as a rubefacient in pulmonary and rheumatic affections, producing little or no pain or inconvenience to the patient.—*Med. Exam.*, from the *Medico-Chirurgical Review*, July, 1842.

Abdominal Tumors.—It may seem strange that the diagnosis of abdominal tumors, which manifest themselves to the touch, and to the sight, should be so difficult and puzzling as it often is. I mentioned some reason for this before: the loose and shifting manner in which some of the viscera of the belly are packed and fastened; their liability to enlarge beyond their natural limits; their accidental dislocations under disease. It would be in vain to attempt even a sketch of the infinite variety of these deviations from the healthy state. Every case of abdominal tumor forms a separate object of study, and must be judged of by its proper circumstances. All that I can profess to do, is to offer you some rough hints on this interesting subject.

Some kinds of tumor result from morbid growths; such are all the varieties of cancer: some from the presence and multiplication of parasites; of which we have examples in collections of hydatids: some are produced by the distension of hollow organs; as where concretions, or fæcal matters, oras seg, lodge in the intestine: some consist in the mere enlargement of parts.

Let us enumerate the principal of these, that you may know what chiefly to expect.

1. There are, I say, tumors from lodgements in the *bowels;* and these are more hopeful than most kinds of abdominal tumors. Sometimes the stomach, or part of the intestinal canal, is distended in consequence of a mechanical impediment to the course of its contents: and this impediment may be invincible.

2. *Ovarian* tumors are very common. Of these I spoke at some length in a former lecture.

3. The *liver* is very liable to enlargement: either from simple congestion of blood; or from the interstitial deposit of adipous matter; or from the intrusion of malignant growths: or from colonies of hydatids.

4. So also the *spleen* swells, from fulness of blood, or from specific deposits in its substance.

5. The *kidneys* sometimes attain a vast size; being occupied by malignant disease, or swollen by pus that finds no vent.

6. Enlargements of the *mesenteric glands;* cancerous degeneration of the peritoneum, especially where it forms the *omentum;* tumors connected with the *uterus;* aneurisms of the *aorta;* constitute other species of abdominal swelling, which I simply mention without further comment.

Now our judgment of the character of a given tumor is naturally influenced by its *place*. In the right hypochondrium, we suspect the liver; in the left, the spleen; in the epigastric region, the stomach; in the hypogastric, the womb; in either flank, an ovary, or perhaps a kidney; in the track of the colon, we guess at fæcal collections.

But sometimes the situation of the tumor fits more than one, or than two, suppositions. Between the ribs and the ilium on the right side we may have an enlarged ovary, a tumid kidney, a distended cæcum. A prominence in the epigastrium may be due to cancer of the stomach, to an infarcted transverse colon, to a ventral aneurism. Above the pubes, the distended bladder, or the enlarged uterus, may equally project. The sigmoid flexure of the colon loaded with fæces, the left kidney exaggerated by disease, a bulky ovary, may either of them occupy the same sinistral space.

Moreover, the colon deviates strangely, and not seldom, from its natural course and position; and the magnified viscera may invade, by their displacement, or by their irregular expansion, the regions that are proper to other organs.

Our conjectures are assisted by the associated symptoms, and by observation of the regular performance, or of the disturbance, of particular functions. Yet here, also, we meet with continual sources of fallacy. Pressure from a tumor without may, as well as infarction within, impede the passage of alimentary matters through the bowels, of urine through the ureters; and cause, in the one case, flatulence and tormina—in the other, retention or suppression of urine. Growths foreign to the liver may, nevertheless, press upon its excretory ducts, and occasion jaundice. And so of other parts and functions. I mean, that the functions prominently deranged are not always the functions of the part occupied by the tumor, but of organs which are secondarily and accidentally subjected to its disturbing influence. Your sagacity will be abundantly tried in balancing the evidence of different symptoms in these obscure, yet palpable, forms of disease: and after all, you will often doubt; and often, when you do not doubt, you will mistake.

Enlargement of the liver may usually be distinguished from other tumors of the right hypocondrium, by *percussion*. Try from the clavicle downwards. At first, you get a hollow sound. Then, a little below the nipple perhaps (for the spot varies much in different subjects), the sound begins to grow dull. If this dulness be traceable, without change or interruption, to the tumor, the inference is strong that the tumor is hepatic.

Any other tumor there situated leaves, most commonly, when the patient is recumbent, a palpable sulcus above it; or a space in which the sound, upon percussion, is different from that which is yielded by the liver.

Percussion helps us to discriminate an ovarian from a renal tumor. When the swelling is large, the intestines lie behind the one, in front of the other; and the sound is affected accordingly.

Tumors that are readily *moveable*, are generally intestinal, omental or ovarian.

A pulsating tumor is not necessarily an aneurism. The healthy artery will lift almost any sort of hard swelling that happens to lie directly over it.

The occurrence of hæmatemesis or melæna would corroborate your belief that a tumor in the right hypochondrium was hepatic—in the left, was splenic.

Even when you are satisfied as to the organ affected, there comes another question, scarcely, in some cases, less difficult than the first—What is the *nature* of the tumor?

Suppose, for the sake of illustration, that our inquiry relates to the liver. If the tumor be large, smooth, roundish, of slow growth, and the general health is not materially deranged, it is, most likely, an *hydatid* tumor. If along the edge and upon the surface of the augmented liver, you can feel large inequalities and projections, and if the complexion and general state of the patient are expressive of failing health, the enlargement is, in all probability, *cancerous:* and if there be other traces of carcinoma in the system, this conclusion becomes almost certain. Small, hard irregularities betoken the *hobnail* liver, which is sooner or later accompanied by ascites. When, without pain or jaundice, the liver of a phthisical patient transgresses its natural boundary, it is, in all probability, *fatty* liver.

By applying a similar method of investigation to other ventral enlargements, you may frequently hit the right scent, and trace the mischief to its true source. To treat the subject in detail would require a volume. I may refer you to a series of papers by Dr. Bright, in the Guy Hospital Reports; where you will find a host of examples, and much valuable information, concerning the most common and the most important kinds of "abdominal tumors and intumescence.—" *Watson's Lectures.*

Use of Moxa in Rheumatism. By John Leney.—The laboring classes in Ireland, from exposure and insufficient clothing, are very subject to local rheumatism. Rheumatic fever is a disease comparatively rare among them. Those forms of most frequent and distressing occurrence are lumbago, rheumatism of the deltoid muscles, and sciatica. In obstinate cases of local rheumatism I invariably apply moxa; there is no application so immediately successful, or which creates less inconvenience to the patient, especially if he be a laborer. At dispensary my ordinary treatment for lumbago is castor oil and turpentine draught, as follows: ℞. Olei ricini, ℥j.; terebinth. syrupi, aa ℥iij.; aq. menth. piperit. ℥j M. Ft. haustus.

The turpentine epithem on flannel to the loins, kept on from twenty minutes to one hour, or until, in fact, the patient exclaims loudly for its removal; it often brings out very minute vesicles, and the cuticle desquamates. Also ten or fifteen grains of Dover's powder at bed-time, the feet and legs bathed in warm water.

In rheumatism of the deltoid muscles, and sciatica, apply moxa by all means, as blisters and tartar-emetic ointment disable the patient, and render him incapable of working. The laboring classes cannot afford to be confined to the house, whereas after the application of moxa the patient may return to his work at once. I shall instance a few aggravated cases in which it proved successful.

James Pender, ætat. 56 years, got sciatica from standing in water, the principal part of a day, washing sheep. He had been confined to the house, nay, to the bed, for six weeks, and had used a variety of means—stupes, blisters, warm plasters, &c., without deriving the smallest benefit. I applied moxa in three places; first, at the origin of the nerve, each application three inches distant, and following the course of the nerve down the limb. This man had eighteen applications. From the first burning the disease descended, so that I had to continue it down the limb to the ankle. Several of the burns continued open for some time. One particularly down the calf of the leg discharged pus for three weeks. This man has been ever since (now five years) at labor without having had any return of the disease.

Judy Cavanah, æt. 57 years, confined to bed six weeks from sciatica of right limb: was burned in four places, on those parts in the course of the nerve which she indicated as most painful. She was immediately cured. That was three years

ago. Last winter she had the disease in the left limb, was confined to bed, and suffered excruciating pain. The moxa again relieved her, being applied in seven places. She had not any internal medicine during either attack.

Bridget Doyle, æt. 48 years, living at a distance of three miles, being informed of her sister's recovery, was brought to the dispensary on a car, and carried in, being unable to put the limb under her; had been confined to bed for fifteen weeks. This woman was burned in three places; and so immediate was her recovery, that she walked home with her husband in two days after. He, laboring under rheumatism of the deltoid muscle of the right arm, which rendered him incapable of working (having been employed in threshing corn), was burned in two or three places, which enabled him to return to his work the following day.

A stout, able-bodied young man, who had undergone a variety of treatment, was blooded, blistered, purged, used sudorifics, and finally had tartar-emetic ointment rubbed in throughout the whole limb, from which he had no relief; from this unlimited application of tartar-emetic ointment, a mass of tubercular swellings remained in the skin, so thick that it was difficult to procure places for the application of moxa; he was burned in three places, and became immediately well, now two years since. He has had no return. I selected the foregoing cases to show its permanent benefit.

June 15th, 1842.—Jane Gaskin, æt. 14 years, has had sciatica of the left limb for five weeks, and been confined to the house from pain and inability to move the limb (the limb is smaller than the opposite). Moxa applied in three places, from the ischium to popliteal space; lost all pain immediately, and was able to return at once to her ordinary avocations. This girl had been blistered, used tartar-emetic ointment, aperients, &c., without any benefit; has had no return of pain up to this day (June 30th).

My experience would lead me to say that if there be not an immediate relief obtained on the first or second application, the case will be tedious, or pass down the limb, as in Pender's case. I am at present treating a case exactly like Pender's, in which the pain has left the parts first burned, and is now taking the course of the posterior tibial nerve.

The way in which I make moxa, and apply it, is the following:—In a strong solution of nitrate of potass, soak a piece of lint, then dry it, cut off pieces the size of the thumb-nail, fasten them at one extremity over the seat of pain by a little adhesive plaster, set fire to the opposite extremity, then blow

on it strongly with a blow-pipe. The pain during the operation is very severe, causing an immediate puckering of the cuticle. Those cases did best, in which, under its influence, a general perspiration broke out.

Had I not mislaid my note-book, I should have been able to have detailed these cases more accurately, and given some cases of affections of the knee and spine in which it proved successful. For the last six years I have been using moxa with undeviating success, and cannot instance a case in which it failed. When first proposed, the idea of being burned was such that it prevented many submitting to it; but now that they have had demonstrative proof of its efficacy, they gladly submit, preferring it much to the application of blisters.—*London Med. Gazette.*

M. Velpeau on Cysts of the Eyelid.—I would say a few words respecting the case of a man, on whom I am about to operate for three small tumors on the eyelid. Of two of these the outline cannot be distinguished, being masked by the third which is more voluminous. This last is evidently a cyst filled with a fluid or semifluid material. The nature of the contents of the two others it is not easy to diagnose, from their size and the thickness of their walls, but from analogy we may suppose them to be filled with a liquid or semiconcrete matter.

Tumors of this description are very rarely cured by any plan of treatment except operation. Occasionally, indeed, they disappear without any operation or treatment. Sometimes this may result from the particular constitution of the individual, as in a young subject who about the age of puberty was freed from an affection of this nature. Or, it may be produced by some other disease concurring at the same time, as in the case of a woman who had three of these tumors on the eyelid, and on whom I had fixed a period for operation. But she came to me some months afterwards, and informed me that she had suffered under an affection of the chest, and the tumors had disappeared. Occasionally they disappear under local applications for their resolution, but we can rarely expect success to attend this treatment, and it is altogether ineffectual when they have attained a considerable developement. They may, however, be safely and with certainty cured by surgical means.

Of the two proceedings, extirpation and incision, the former, employed by most surgeons, appears to me a tedious operation. It is necessary to dissect with caution, and if it be a

cyst, whatever care is taken, it will be difficult to avoid opening it; it then is necessary to excise it entirely, and to apply cauterization, or there will be a considerable chance of return. If the contents of the tumor be concrete, the operation is comparatively easy; but, otherwise, I think incision is preferable: it is less painful, and quite as successful. Some persons in practising this operation hold the tumor with the fingers, others place an elevator beneath the eyelid. I employ simply two pair of forceps, of which one is held by an assistant; I myself, holding the other in such a manner that the eyelid is rendered tense between them, and raised from the globe of the eye. I make the incision, expel the contents, and apply nitrate of silver to the interior of the cavity. There is sometimes slight consecutive inflammation, and the eyelid swells a little, and soon an eschar is detached and the cure accomplished in eight days. A piece of lint dipped in saturnine lotion or cold water is the only dressing employed. I should, however, observe, that by extirpation the cure is more prompt; a piece of dressing is placed on the wound, and the whole is terminated.

After these observations the operation was performed: M. Velpeau incised the different tumors and cauterized the bottom of each wound.—*Lond. Med. Gaz., from a Clinical Lecture by M. Velpeau, in Gaz. des Hopitaux.*

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FLUOR SPAR.

Dr. E. R. Roe, of Shawneetown, Illinois, wishes to inform scientific gentlemen through our journal that he can supply orders for *Fluate of Lime*, addressed to Drs. Roe & Badger at the place of his residence, if properly accompanied. He writes us that, since leaving Louisville last spring, he has received requests from various gentlemen to send them specimens of this mineral. He is prepared to furnish specimens to as many as may desire them. The mines are within about 20 miles of Shawneetown, in a wild, unsettled region of country. He says he has "spent some time and much pains in collecting specimens beautifully crystallized, and tinged with every shade of lilac and pink, up to pure transparency." Dr. Roe is a gentleman of science and industry, and may be depended upon for the faithful execution of all that he proposes. Y.

"REPORT ON THE MEDICAL COLLEGE" AGAIN.

In our last number we noticed briefly a "Report" made to the City Council on the subject of the Medical College, in which the conduct

and motives of the Faculty were assailed, and which, after having been rejected by the Council, was published and sent abroad over the country. We stated at the same time that the Faculty were preparing a reply to the unfounded statements and unjust imputations contained in this document. That reply is before us now, in a pamphlet setting forth the "origin and present condition of the Louisville Medical Institute." It is a plain, unvarnished statement of facts, temperate in its tone—very temperate indeed under all the circumstances. As copies of it have been sent to the patrons of the Journal, we think it unnecessary to indulge in copious extracts from it, as was our intention, and shall therefore only advert briefly to one or two points connected with the matter.

The Dean, in the pamphlet before us, dives at once to the source of this movement, and of course looks beyond the Committee. He says:

"It is known to the citizens of Louisville that there is a party in the city who have for some years been endeavoring to produce discontent with the management of the Medical Institute of Louisville. The Managers and Professors have borne their repeated expressions of dissatisfaction, publicly and privately made, and their attempts to take the school out of their hands, with quite as much patience as is commonly shown in similar circumstances. Some indeed have manifested a natural and justifiable indignation at these unworthy efforts and schemes; but they have avoided a public expression of it, and no public defence has yet been made. They have felt extreme reluctance to a controversy with men who appeared to be proceeding from outrage to outrage, in hope of provoking one, from which they hoped to gain notoriety at least, and in which they had little to lose. But inasmuch as these men have at length succeeded in misleading some of the Council so far as to have their views presented to that body in the shape of a report, which has thus acquired some appearance of importance; although the report was rejected, it has been thought advisable by the Faculty, to place the matter in its true light before the friends of the school, some of whom may be misled into believing the statements made in the report.

* * * * *

Indeed the temper of the rejected report and the character of its statements are such as to lead the Faculty to the conviction that the enmity which prompts these movements is not to be mitigated by time nor softened by forbearance; and some extraordinary acts of those who can no longer be considered in any other light than that of determined enemies of the school as now constituted, as well as the inquiry of a number of citizens, have led them at length to the opinion that it is time to check the influence which has misled some of the Council, and which left unnoticed might mislead others.

* * * * *

These men indeed sometimes deny that they have any intention, in these efforts to obtain possession of the management of the Medical School, to disturb the present professors. But it is not to be believed that, if after years of intrigue they could by any possibility get the administration of the school into their hands, they would cease until they attain their main object, openly avowed two years ago, the possession of the chairs. It is not to be believed that men who have arranged a plan for delivering lectures, and have asked for the use of the Medical Institute for the purpose, would fail to take possession of it if they should dispossess the Managers whose sense of duty and propriety compelled them to deny them. It is evident that the object was, by succeeding in carrying the report, which they without foundation supposed would give them possession of the Institute, to do that which they openly proposed to do in 1840, eject the present Professors.

Such are the motives which led to the statements in the rejected report, and to the improper use that has since been made of it. * * * And so completely have the members of the committee entered into the feelings of the inimical party, that, according to the statement of the editor of the Gazette, which has not been contradicted, they had a great number of copies of the report printed, with the ayes and noes attached, in a form evidently intended for distribution; and one of the committee has stated to a gentleman, although he was told that the information was sought for in order to be used, that the publication was made in consultation* with Dr. Bell, Dr. Bullitt. and Dr. Flint, and from the interest they manifested in it he supposed they would be willing to pay a part of the expense if called on."

As it respects the transfer of the Square, buildings, &c., from the occupancy of the Medical Institute, which was the ostensible object of the committee, the report, so much of it as is devoted to that object, proceeds on the assumption that the Louisville College is the corporate body alluded to in the deed of trust by which the Managers of the Institute at present hold the property. Now the rejoinder of the Faculty makes it about as plain as words can make it, that this College and its trustees were no more in the minds of the City Council or of the people at their public meeting than an Indian wigwam and its inmates!—not a bit more! No reference was made to them in any way, nor can the language of the indenture or the resolutions of the citizens, by any legitimate philological torture, be made to refer to them. And yet this "elaborate report", whose statements, according to the editor of the Louisville Gazette, are "undoubtedly authentic", says, "nobody

* The member of the committee here referred to, has since stated in one of the city prints, that the expression "made in consultation with," &c., is a misapprehension.—Ed. Jour.

thinks nor has thought of establishing any other school in Louisville!" The following extract from the exposition of the Faculty contains a summary of the evidence offered by them on this point, and shows on what grounds the transfer is to be made.

"The proposition to request a transfer of the management of the school from the present Board, rests on the ground of an asserted right arising from the donation of the Council.

The right of management was in the Board of Managers of the Medical Institute of Louisville years before the donation was made. It was the knowledge of the powers they possessed which led the Council to make the donation, and was the avowed ground on which it was made. The donation therefore gives no right of management to the Council.

What then are the legitimate claims of the Council upon the school? They are those only which arise out of the terms of the donation when accepted: for it is the acceptance of a donation which subjects the receiver to the restrictions contained in the terms of the gift. Beyond these the claim of the donor does not extend.

In the present case the terms of the donation which was accepted by the Managers were these, beyond which the Council have no claim. 1. That the property should be used by the Managers for the purpose of carrying into effect their previously possessed powers of establishing a medical school: and, 2dly, that in the event of the Council's carrying the views of the citizens fully into effect, by erecting other college buildings on University Square, and obtaining a charter for a university, the Managers are to transfer, if the Council request it, the square, buildings, &c., to the trustees of the university.

The first authorises them in case the property is used for any other purpose than that of a medical school, to reclaim possession of it. But as long as it is used for the purpose for which it was given, they cannot move in the matter. How far a great failure notwithstanding the efforts of the Managers to raise up a school sufficient to satisfy the expectations of the donors, might authorise the latter to interfere, need not be considered; seeing that in the present case the utmost expectations of the whole city have been far exceeded.

The second authorises the Council to build other college buildings on University Square, and having obtained a charter, to require the Managers of the Medical Institute to transfer the square, &c., to the trustees named in it. Before this is done there is no ground for the request.

In case however that the wishes of the citizens expressed in their meeting in 1837 should be carried into effect and buildings be erected for the other departments, and the managers of the Medical Institute should be called on to reconvey the square, &c., it is the property only that is to be reconveyed, by right of the reservation in the deed of donation; which can go no farther than to call back what was

granted; viz. the property, not the right of management, which was in the Board of Managers before the donation was made.

When that time shall arrive it will be a matter of consideration on what terms the Medical Institute of Louisville, called by anticipation the medical department of the intended university, shall really become so. These will no doubt be such as shall be agreed to by those under whose charge it has increased beyond all expectation and beyond all precedent, and in all probability will continue to increase."

One of the most calumnious charges made against the Faculty by the authors of the rejected report, is that of having embezzled the funds of the institution. But a small portion, they say, of the monies derived from the matriculation fees have been devoted to the purposes of the school, "the residue having gone with all the other revenue of the establishment into the Professors' pockets." So far from this being the case, it appears from a detailed statement of the expenditures given in the pamphlet, that "the expenses of the School have exceeded by several thousand dollars the amount of matriculation fees received;" in fact, that the Faculty have taken money out of their pockets to the amount of $8,000 over and above that fund.

But perhaps the most perfectly gratuitous suggestion contained in the report, and at the same time the one most ruinous and destructive to the school, is that the proceedings of the Board of Managers have been to such a degree informal "as to vitiate and nullify in a legal sense all their recent acts"; and of consequence, that "the appointment of professors, the financial arrangements of the concern, the distribution of collegiate advantages, and the conferring of degrees, are done without authority of law, and of course are invalid and worthless to all interested." "It appears," says the report, "that there are still living three of the original corporators of the Medical Institute, who have neither participated, nor been summoned to participate in any of its transactions for the last four or five years—a portion of the associates having proceeded to exercise all the corporate functions, without any regard to the equal rights of the excluded members."

To this most injurious statement, which strikes directly at the very existence of the institution, and yet is made under the garb of the most disinterested friendship, the Dean, after directing attention to the early history of the Institute as previously given by him in the pamphlet, and which shows the utter groundlessness of the assertion, replies more particularly as follows:

"The three surviving *members* here spoken of, are medical men, who nine years ago excluded themselves from all participation in the management of the Institute, by passing the regulation that no medical man should thereafter act as manager. This, being managers at the time, they had full authority to do by the act of incorporation in 1833; and the Legislature confirmed it by the act of 1835 as the permanent law of the school.

'The portion of the associates (spoken of) who proceeded to exercise all the corporate functions,' are four still surviving managers of that Board, which the old three abovementioned (and their then associates) constituted nine years ago; together with those who have been since elected. In electing new Managers instead of those whose places were vacated by death, resignation or removal, they were debarred from choosing any of the surviving three medical men, by the law of the Institute made by these three themselves: and were compelled to appoint the same men managers because there were no others eligible, as the three had themselves arranged the matter when they brought in the non-medical men expressly to be made Managers.

If the three had been present at elections they could have carried no candidate, as they were from the beginning in the minority, six members, medical men, having chosen nine men not medical, and made them managers, If they had even had a majority it would have made no difference; because there were no others to choose than the non-medical men who constituted the old Board. Neither would a majority have enabled them to bring in others to make managers of; because the Board had the right by the original act of 1833 to determine the mode and manner of admitting new members, and how they shall cease to be such; and no one could be admitted unless nominated by the President or Moderator. By the operation therefore of the original act of incorporation in 1833, and the regulation made by the three medical men, the result was the same whether there was an election or not; viz. the perpetuation of the office in the hands of the Board."

The rejected report contains other accusations and complaints, which do not involve perhaps any direct criminality on the part of the Faculty, but still are of a kind to excite prejudice in the minds of the uninformed. These we shall not notice further than to say that they are fully refuted by the pamphlet, and shown to be as destitute of foundation and as little worthy of confidence as those to which we have adverted.

That the committee should have preferred the grave charges contained in this paper confessedly without "authentic information," is certainly most extraordinary, and comports badly with their claim to sincerity. And it is equally extraordinary that, after a majority of their fellow council-men had cast the thing of shreds and patches from them as though it were something unclean, they should proceed to have

it printed, and threaten the Faculty with its distribution if an attempt were made to discredit it! Would it not seem, indeed, that they feared the exposure which has followed?

Conceding, however, to the Committee the merit of sincerity, the three individuals who manifested so much interest in this report, as to lead to the supposition that they would pay a part of the expense of publication if called on, must be regarded as the true culprits. One of this worthy triumvirate, Dr. Flint, was, it will be recollected, at one time a member of the Faculty, and of course had "authentic information" on all the matters involved. No man, therefore, knew better than he did the utter falsity of many if not all of the charges contained in this report. The Board of Managers, according to this document, is illegally constituted. Yet Dr. Flint accepted office under this same body, and most reluctantly yielded up his professorial privileges to it. As a member of the Faculty he was well aware that the expenditures during the first three years far exceeded the amount of matriculation fees, and actually joined in obtaining a considerable loan to supply the deficiency. Yet he has not hesitated to give currency to a paper charging the Faculty with having appropriated to their own uses the money thus derived from matriculations. By what code of ethics this gentleman's conduct is governed, it is difficult to imagine. Surely not by that which obtains among honest and upright men. What apology he may frame for himself out of the circumstances attending his ejection from the Faculty of the Institute at a former period, we are unable to say. We can conceive of nothing, however, that would justify him in pursuing a course of conduct such as is exposed by this pamphlet, and which must be looked upon not merely as disingenuous but disreputable. C.

HOMŒOPATHY.

We have been considerably entertained by some observations on this grandest of all grand humbugs, which we find in Lea & Blanchard's Medical Intelligencer copied from the *Physiologie du Medicin* of Louis Huart. The writer after telling us that the essence of the Homœopathic doctrine is contained in the aphorism, *similia similibus curantur*, gives some instances of miraculously prompt cures, and proceeds in this wise—we translate literally:

"It is not only men that homœpathic physicians treat with the success described above. They likewise bestow their cares upon

quadrupeds generally—even upon asses. However, we do not think they do so always in pursuance of their famous maxim ***similia similibus***—that would really involve too much of Christian and homœopathic humility."

Then follows an illustration which is inimitable of its kind: a man with an enormous quantity of cravat, and proportionally small quantity of hair, is seated, with spectacles on nose—which last feature has a most inquisitive perk—peering intently on the protruded tongue, and feeling the pulse in the sinister fore-leg of a long-eared animal that lies in graceful reclination before him, with a most dolorous and *asinine* expression of countenance.

"Another fundamental point of the doctrine in question is, not to administer internal remedies except in infinitesimal does. This has some advantage, for without it the homœopathic practice would be much more dangerous to the public."

There is a good deal of truth, mayhap, in that last observation. After a lively and graphic description of the preparation and administration of one of these small doses, our author proceeds:

"With a lozenge [*tablette*] of jujube, the homœpathic physician would form potions to cure a whole regiment of cuirassiers affected with the whooping cough."

But the richest conceit is the following:

"I firmly believe that the homœopathic doctrine was originally invented by a physician that had suffered a refusal at the hand of an apothecary's daughter; and from the anger of this frantic lover has arisen the system which must inevitably ruin those philanthropic establishments where they sell liquorice and senna at a profit of eight hundred per cent!"

What say you to that, reader? Is it not droll, and above all, is it not exceedingly Gallic? Apothecaries' daughters should be more circumspect.

"Hence the pharmaceutists have sworn a mortal hatred to the homœopathists, who have been obliged to prepare and vend for themselves their little packets of white powder.

Nobody would find much fault with this, if unfortunately these small, infinitely small packets of white powder did not cost as much as the great bottles of drugs under the old system!

If this is complained of, the homœopathist answers: *Every body must live!* Unfortunately this phrase is not applicable to their patients!"

Can any one tell us where to procure Huart's *Physiologie du Medicin?* It must be a rare book, racy and original. The spirit of Moliere undoubtedly dwells in the author. C.

THE

WESTERN JOURNAL

OF

MEDICINE AND SURGERY.

NOVEMBER, 1842.

ART. I.—*A Case of Compression of the Brain, reported to the Medical Society of Tennessee, at its 13th annual meeting in May*, 1842. By JOHN R. WILSON, M. D., of Davidson County.

Nancy, 32 years of age, the servant of Wm. H. Robertson, Esq., of Rutherford County, during the seventh month of her sixth pregnancy, was attacked with severe pain in the left side of the head, involving the temple, eye, &c., and with pain in the lumbar region, but not severe; her bowels were costive, tongue furred, pulse full and strong, appetite capricious. I bled her freely from the arm, administered a dose of castor oil, had a cold wet towel applied to her head, which had more than ordinary degree of heat. I visited her next day and found all the symptoms somewhat relieved; ordered light diet, and a repetition of the oil when necessary to prevent constipation. This course

enabled her to pass her time with only an occasional recurrence of pain in the left side of her head, to which she said she had been subject many years, until about three weeks after the birth of her child, at which time I was called to see her again and found her suffering from pain as in the first attack, with but little arterial excitement, surface cold and clammy, tongue much furred, left eye slightly inflamed, no appetite, tinnitus aurium, and bowels confined. An emetic of ipecac. was administered at noon, 15 grs. blue pill ordered at bed-time, to be followed by a dose of oil next morning. During the next morning I visited her again; the oil had operated, but every symptom was worse—a hard tumor, circumscribed and slightly elevated, was discovered on the left side of the os frontis; near the junction of the parietal bone; there was strong full pulse, hot skin, occasional aberrations of mind, intolerance of light and jactitation. I bled her copiously, but without any relief of pain; gave 20 grs. calomel, shaved the tumor, and ordered oil in six hours, with warm fomentations to the tumor and temple. I visited her next day and found all the symptoms better except the pain in the tumor, in which she complained of severe pain and throbbing. I applied a blister plaster over the tumor and administered a dose of senna and salts. Next day the blister had drawn well, and the tumor being now soft, I opened it by a large incision, when it discharged a considerable quantity of purulent matter. An emollient poultice was applied, and directions were given to repeat it, with a view to keeping the orifice open.

For two days after the opening of this tumor the patient appeared relieved. On the third day she became extremely restless and unable to sleep, with want of feeling in the right hand and arm, which gradually extended to the lower extremity of the same side. During the ensuing night complete paralysis of the right side took place, attended with laborious breathing and stupor. The patient returned no answer that could be understood to any question asked; pulse slow and oppressed, but regular. A free crucial incision was made through the tumor, and on examination a portion of the per-

icranium was separated from the skull, and a small hole was discovered sufficient to admit the end of a probe through the first table of the bone, but no farther. The trephine was applied immediately, and as soon as a portion of the bone had been removed, four or five ounces of purulent matter were discharged. This was in a short time followed by a return of the power of distinct articulation and of a correct sense of hearing, and the patient was soon able to answer any question asked. The bone had lost its natural appearance as far as the dura mater was detached, which was to some extent, and several crowns of the trephine were taken out, so as to remove as far as possible all the diseased bone, which resembled wood of very hard texture irregularly worm-eaten. The dura mater had a dusky red appearance. Simple dressings of the ordinary kind, with gentle laxatives and light diet, soon enabled the patient to recover her health, which has continued without much interruption up to the present time—a period of more than 14 years. I would further remark that the patient had not received any mechanical injury on her head, but was of a family disposed to scrofulous diathesis, some of her brothers having since died of that disease.

May, 1842.

Art. II.—*History of a fatal Case of Chronic Spinitis.* By Calvin Smith, M. D., of Toledo, Ohio.

On the 8th of October, 1841, Dr. Perkins of this place and myself were called, as consulting physicians, to visit Judge ——, aged 43 years. By enquiry into the history of his case, we learned that about the first of the previous June, he had, for a few days, labored very hard in his warehouse at his writing desk, as well as at other work, such as handling boxes, &c. About this time he was taken ill with rheumatism, and a considerable reduction of voluntary motion in his extremities, more especially the inferior. He called in his family phy-

sician, who bled and purged him freely, applied a blister to the spine, which was not, however, continued for any length of time, salivated him, and also had recourse to strychnine. Notwithstanding this treatment, the paralysis increased, until he had very little power over his extremities. At the end of three weeks it became necessary for his attending physician to leave town, when the patient passed into the hands of another, after which there was not the least attention paid to the spine —the new physician, together with the patient himself, not believing that the medulla spinalis was in the least affected. Recourse was then had to showering the limbs with cold water. In July there was a short period when he so far improved, that by the aid of a cane he could walk around the room, and even to a little distance out of doors, which gave him much encouragement; but his hopes were soon blasted by a relapse, and in a few days the paralysis was as great as it had been at any prior time. The showering of the limbs with cold water was now resumed; they were also rubbed with various liniments, and electricity and *animal magnetism* were resorted to. Tonics were likewise given, and in short, he tried many things which *quackery* only could invent, to the utter neglect of the spine and all proper constitutional treatment.

I was first called on the evening of October 8th, and found the patient very faint, with a great difficulty of breathing, and frequent eructations, which I was led to believe were owing to acidity of the stomach, in consequence of fermentation of food; particularly as I found his appetite was good, and that he had been allowed whatever articles he chose to eat. He was so much exhausted that I found it impracticable to give him a thorough examination, or indeed to examine his spine at all, that night. I therefore, for the purpose of correcting the acidity of the stomach, and relieving his faintness, recommended a dose of carb. ammon., and left him for the night. At 2 o'clock, P. M , on the 9th, I again called and found that he soon became more comfortable on taking the ammonia, and had passed a tolerable night, and that he was free from the difficulty of breathing, which he labored under the night pre-

vious, and able to undergo the necessary examination. I first turned my attention to the paralyses, and found the power of volition wholly gone in his upper and lower extremities, with the exception of the left fore-arm, which he was able to move imperfectly. He had no power over the abdominal muscles, and consequently his stools were involuntary, or only brought about by the natural action of the intestines wholly independent of the will. He could, however, void the contents of the bladder at will. There was some degree of sensibility in the extremities, but it was very much impaired. His mental powers were apparently in a healthy condition. He had the power of moving the head perfectly, and his shoulders to some extent. The powers of speech and deglutition were perfect, and so also were all the senses. He had experienced some pain a great proportion of the time in the back and extremities. The pain had, however, very much abated at this time, but the greatest caution was required in turning or moving him in bed, on account of the pain it produced. In view of the above symptoms, I was naturally led to infer that the medulla spinalis was materially affected, and on examination by pressure found that much pain was produced when pressure was made upon the cervical vertebræ. There was also some tenderness along the dorsal vertebræ, but nothing like the amount which existed in the cervical region. On pointing out this tenderness to the attending physician, he for the first time became satisfied, that the spinal marrow was affected—having, till now, with several others who had been called to see the patient, attributed the difficulty to some inexplicable affection of the nerves. I advised immediate counter irritation along the spine, with such constitutional treatment as might be indicated at this stage of the disease, together with a regulated diet.

Dr. Perkins concurring with me in opinion, the tartar emetic plaster was applied along the spine. On the morning of the 11th I was again called, and requested to take the whole charge of the patient myself. On entering the room I found there was much difficulty of breathing, which in fact brought

into action the sterno-cleido-mastoid muscles. The other symptoms were much as they were on the 9th, with the exception of his being somewhat more exhausted. I was now convinced the spinal marrow was affected as high as the origin of the respiratory nerves, and also felt satisfied that he could not long survive, and might drop off very suddenly, of which I apprised the friends.

I resolved, however, to leave nothing undone which might possibly be of any avail. I removed the plaster from the back, which I found somewhat red, and applied nitric acid for the purpose of producing an immediate effect.

The respiration, however, became more difficult, continually bringing into action more and more of the respiratory muscles which in a healthy state are dormant. The strength gradually failed, although stimulants were admistered freely, until about 11 o'clock, P. M., when he expired.

His mental powers and the functions of speech were perfect to the last. He voided his urine not to exceed twenty or thirty minutes before he ceased to breathe, and appeared to have perfect control over the bladder. He expired without a struggle, and in fact every thing had just the appearance of the respiratory muscles quietly ceasing to act. The bowels moved once or twice a day, without the aid of laxatives, from the time I saw him till his death, which I was informed had been the case for some time previous. The stools did not vary in appearance from what we find them in perfect health. He did not lose control of the bowels until the last two or three weeks of his life. After this time he had a perfect sensation of the movement of the bowels, but had not the least control over them or the sphincter. It was suggested by a gentleman, who had been his family physician prior to coming to this section of country, and who had seen him during his last illness, that it was a scrofulous affection of the spinal marrow. He was led to this opinion from the fact that some seven or eight years prior to this attack, the patient had a carious affection of the sternum, from which however he recovered as soon as exfoliation took place, and from which time he has not la-

bored under any inconvenience. The patient attributed this affection of the sternum to his having been for several years previous constantly engaged at the writing desk. On inquiry I found that there had never been the least symptoms of a scrofulous diathesis before the system had become perfectly matured, and that the only thing which could lead one to suspect it, was the affection of the sternum. From all the facts, I was led to believe that the remote cause was not of a scrofulous character. I will also here remark, that four or five years previous to this last illness, he had frequently suffered very much with the common intermitting and remitting fevers which have been so prevalent in this section of country, and also, that he had taken very liberally of tonics, particularly the sulph. quinine, and I strongly suspect without proper attention having been paid to the hepatic system.

On examination after death, the vertebræ were laid open from the occiput to the lumbar region. On puncturing the membranes which enclose the medulla spinalis, water gushed out very freely, and they were found to be considerably injected with blood, a sufficient proof of inflammation. The vertebræ and interosseous substance were perfectly healthy, which to my mind clearly established the fact, that the disease was not of scrofulous origin. I desired to examine the brain, but did not, as there was some objection to it on the part of the friends.

August, 1842.

Art. III.—*Observations on Scarlatina Anginosa, as it appeared at St. Clairsville, Ohio, in* 1833. By Thomas Carroll, M. D., now of Cincinnati.

Before I bring to the reader's consideration the disease which is the principal object of this essay, I will make a few observations on the anginose affections, with scarlatina, that preceded the year 1833.

My own observation will not enable me to say much about those diseases anterior to the year 1829; and *nothing* farther back than 1824. From that year until 1829 scarlatina was not observed by me, but angina maligna occasionally prevailed, and often proved fatal.

During the winter of 1828—9 scarlatina simplex first made its appearance at St. Clairsville, and in a few localities in its neighborhood. During that winter I did not lose a single case, though the efflorescence in most cases was general, and somewhat intense in its character. Indeed, my success was such as to inspire me with great confidence in the treatment of that too often unmanageable disease; but alas! time soon brought down my arrogance, and taught me, that, like epidemic cholera, this disease often sets at defiance the best directed efforts of our art.

From this period until 1833, scarlatina almost constantly prevailed, either at St. Clairsville or in various surrounding localities, as well as at different places in the belt of country lying between the Ohio and the Muskingum rivers, where it occasioned great mortality, though at St. Clairsville but very few fatal cases occurred. Indeed that town and its vicinity has at no time suffered with the same severity as many other places around it. Of those who died in 1833 many were adults, among whom were persons of such note in society as to attract attention to the prevailing mortality, and perhaps make it seem greater than it really was.

This epidemic of 1833 is that to which I desire to direct the reader's attention, as its character was different from any type of scarlatina I have ever seen. It commenced in February, declined in July, but did not terminate until August. Its character changed with the state of the season, being more intense in hot than in cool weather. This remark will at least hold good with regard to those cases which originated in cool weather, which instantly got worse, if any considerable rise of the thermometer suddenly took place, especially if they had been sick from one to four or five days. So well did I understand this matter, that on one occasion, when a sudden change

of temperature from cold to warm occurred, I predicted its sinister effect on my patients; and within a week I lost five adults in town. This fact, in connexion with others, warned me to avoid heated and close rooms in the treatment of this disease; indeed, I am not certain but heat is as injurious in the this disease as in small-pox. When scarlatina commences during hot weather, the cases often do well; or if it commence in warm, and terminate in cold weather, I think it is not aggravated by the change.

The first appearance of the disease in February was characterised by inflamed fauces, elevated and reddened papillæ of the tongue, with swelling of the mucous membrane generally, slight salivation, headache, rigors, fever and delirium. The fever, in most cases, was accompanied with an unusually strong and frequent pulse, and great heat of the surface, but almost altogether without eruption of the skin. This condition lasted from four to seven days; when, if the patient did well, the symptoms abated, and he was convalescent by the tenth day. When the result was unfavorable, a singular, and to me, new set of symptoms supervened with continued delirium, and other signs of inflammation of the brain.

As the season advanced, these symptoms became united with other and more serious ones, that frequently bade defiance to all the efforts made to stop their fatal tendency; and were mostly brought on by imprudence of the patients after they had pretty well recovered from the severity of the disease, by active treatment at the outset followed up by abstemiousness and quiet. When by these means the patient was so far restored as to go about the house and resume his usual habits, some irregularity in eating and drinking, or the exposure of himself to cold and wet, or even washing in cold water, has brought on the following symptoms: Fever, with a revival of the inflammation of the fauces, frequently travelling up the posterior nares, and involving the whole pituitary membrane, and the sub-mucous cellular substance; and at length attacking the skin above. The first appearance of inflammation of the last, was mostly observed on the bridge of

the nose; the centre of it being at the termination of the ossa nasi, and forming a belt across the nose, about three fourths of an inch wide, with an apex resting on each cheek, though sometimes there was but one. This inflamed belt either crossed one or both cheeks; and when it only travelled across one side of the face it invaded but one ear; but if the contrary, both were involved in the difficulty. The swelling spread from this belt, and mostly the whole face and scalp became involved in the tumefaction; though, where the inflammation spread on one side of the nose and only crossed one cheek, but one side of the face swelled, and no part of the scalp inflamed. The color of the skin was red, more or less approaching a venous hue, and during a few of the first hours had a polished aspect: but vesication uniformly resulted in a short time, and spread over the face and ears, but did not invade the hairy scalp to much extent; which union of the vesication and swelling completed every characteristic of erysipelas, and was accordingly treated as such. Abscesses also formed about the head, and I know no instance of death when they arose.

This condition in those who survived it, lasted about ten days before the swelling materially moderated; when it gradually yielded, and the patient recovered his usual health; and, so far as I saw, no return of erysipelas took place. But nearly half of those who were thus diseased, never recovered; inflammation of the brain having eventually carried them off, and that mostly within the first week or ten days from the commencement of the erysipelas.

In most cases the lips and tongue swelled until articulation was either difficult or impossible, and the tumefaction extended over the face and scalp until the eyes were so buried beneath that vision was impossible. Abscesses frequently formed either in the upper eyelids or in the scalp, each of which contained about a tea-spoonful of pus when fully matured. It is singular that no case of death took place, as I have just said, when these abscesses formed about the head; which was not the case when they supervened on the extremities. Other

and more appalling symptoms occurred in a number of cases, the result of which was the death of every one in whom they were developed. These consisted in swellings of the extremities that mostly terminated either in suppuration or mortification. They were confined to the cellular substance both exterior to, and beneath, the fascia surrounding the muscles; and in one or two instances, in the larger joints. In the cases which terminated in suppuration, healthy pus was not formed; but a thin darkish matter approaching to green, was the result. Amidst this matter there was, in nearly every case, a quantity of fibrin closely adhering together, and forming a substance more or less extensive according to the size of the abscess. Their common length was from three to six inches; their width one and a half or two, and their thickness from three to five lines. These substances adhered so closely together, that I generally succeeded in drawing them out of the orifices made for the exit of the matter. To give a clearer idea of the nature and effect of these abscesses, I subjoin the two following cases.

A gentleman, aged about 65, was taken in the usual way, and did not at first seem very ill. He would take no medicine, nor submit to depletion by bleeding, and indulged in eating whatever he wished. He continued to have fever after the time it usually subsided, and at the end of three weeks he suddenly complained of pains in the calves of the legs, which on examination were found to be swollen, particularly on the inner sides. Soon after, both his fore-arms inflamed and one of his elbows, and all went on to suppuration; abscesses forming on the inner side of each leg and arm, and one round the elbow joint. When the matter was discharged four fibrinous substances were drawn from four of them. After this, they all healed, and a sixth formed low down on the right side. This abscess was very large and extremely painful. It connected itself slowly with the walls of the abdomen, which rendered it easy to reach the matter with an instrument; but I long hesitated before I opened it, as I thought that death would soon follow the evacuation of the pus. The ear-

nest solicitations of the patient, however, overcame my hesitation. I opened it, and my patient within seventy-two hours paid the debt of nature, after having suffered a most painful disease during three months. About ten days from the commencement of this patient's illness, his wife died of mortification of two of her extremities by the same disease.

A lady aged 35, had at the commencement the usual symptoms, and in the course of a week seemed to be rapidly recovering. She returned to her ordinary occupations, which exposed her too much. The febrile symptoms recurred, and the inner surface of each fore-arm inflamed and suppurated. I opened the abscesses, and drew out of each strips of fibrin. These abscesses eventually healed, and the patient again seemed to be doing well; but, of a sudden, pain and swelling of the side of the head, with fever and delirium set in; and before suppuration took place death ensued, in about five weeks from the first attack.

Five cases terminated in mortification. In two of the patients the arm was its seat; and in one of these it also attacked the leg. Three others had one leg each mortified. The first of those affected in the arm seemed to have had the mortification brought on by bleeding, as pain at the bend of the arm, with inflammation of the orifice, and swelling of the limb, took place within 24 hours after the operation. The patient lived only three days after the commencement of sphacelation, which within that time involved the greater part of the arm. In a few days afterwards, however, I was called to prescribe for the sister of this patient, a lady advanced to the 65th year of her age, though ten years younger than her brother. When I examined her, my impression was so strong that she must be bled, that I proposed taking blood; but my patient refused, on the ground that she thought her brother had fallen a victim to bleeding. My reasons for wishing to bleed were, that she had an uncommonly strong pulse, pain in the head, hot and dry skin, and soreness of the throat; but the repugnance of my patient, and the melancholy termination of the last case caused me to yield; and I made the best prescrip-

tion I could under the circumstances. On the following day, to my great astonishment, I found the arm, out of which I had intended to draw blood, considerably swollen and very painful, and the leg of the same side in the same condition. The color of the limbs was slightly venous; indeed, venous congestion was very obvious. At my next visit, 24 hours after, I found both limbs affected with incipient mortification. Another day brought about a fatal termination.

In the three remaining cases, the arms escaped, and but one extremity was affected in each. Great pain was experienced in the inflammatory stage. This state of suffering lasted but a short time, probably not much over 36 hours, in any of the cases; when other symptoms, less painful but equally distressing, supervened. One of these cases was that of an intemperate colored man, about 35 years of age. Another that of a lady advanced to the sixtieth year of her age, whose constitution had been broken up by long continued disease. The third and last had sound health up to the time of her fatal attack; she was a poor and laborious woman. They all died.

The inflammation in the above cases was no doubt of the erysipelatous character; and its malignity arose from its close connection with a masked form of the scarlatina.

Not less than five pregnant females became affected with the epidemic of 1833, and one case of a similar kind occurred in 1834, of which all but one proved fatal about the fifth or sixth day.

The first case I saw in consultation. The lady had been ill three or four days, and had been taken with a violent rigor, which was succeeded by intense fever, accompanied by pain of the head, back, throat and chest; and after the continuance of this for some hours, another rigor followed, which was again succeeded by fever. When I first saw this patient I determined in my own mind that the death of the fœtus had taken place, and that abortion would not follow without proving fatal to the mother. Abortion did not take place, but death supervened in six hours. She was advanced in pregnancy to the fifth month. The death of the fœtus took place in every

fatal case but one, before miscarriage. Abortion was the result in only two cases; and one child was born at full time on the third day of the disease. This child is still living, and has in the main good health. On the evening that the child was delivered the mother observed an inflamed spot on her fore-arm, just above the wrist, in which the cellular membrane was involved. When she observed it, she said that she would not live, as all who had been thus affected had died; and her prediction was verified in a few days. The lady who recovered had visited her friends in town, some of whom were affected with the worst forms of the disease. She became but slightly affected, and by taking much care, recovered and gave birth to a fine child, some months afterwards.

Of the contagious nature of this disease I have but little doubt, and what is most singular it propagated the scarlatina anginosa. For instance, a gentleman and lady had that disease, combined with erysipelas, which proved fatal. Their children, who lived at some distance, visited them, and some days after their return home several of them were taken ill with it; but none of them had erysipelas in any form; those, however, of their neighborhoods, who visited them, and became diseased in a similar way, in two or three instances had erysipelas of the face; yet no such disease took place among others in that region, who were not exposed to the contagion.

This same gentleman and lady were visited by a lady who lived in another section of the country; and she also had the affection with erysipelas of the face. Her daughter had the disease combined with erysipelatous inflammation; while several others of the family had it uncombined with that dangerous symptom.

At a number of places through the country, scarlatina existed in its simple or malignant form; but in no place did the same phenomena present themselves as in St. Clairsville and its immediate neighborhood. I saw two cases, however, during the same season, in which two thirds of the nose was involved in an inflammation that had travelled up the schneiderian mem-

brane from the fauces. There was, however, but very little swelling and no vesication. In both cases death was the consequence.

It not unfrequently happens, that a tendency to erysipelatous inflammation is manifested in the eruptive stages of scarlatina, as a result of bleeding or blistering. As an evidence of this I will relate the following case. My friend and former student, the late Dr. W. Wright, of eastern Ohio, informed me, some years ago, that he had, during his residence in Guernsey County, bled a patient laboring under scarlatina, and that the orifice healed in about the common time; but the patient did not recover with the usual rapidity, and in about two weeks the orifice which had seemed entirely well, suddenly ulcerated, and the inflammation rapidly spread and soon involved the whole arm, which terminated in mortification.

We now arrive at the treatment of this disease, which was at least characterised by energy. I usually commenced by bleeding the patient to approaching syncope, after which I subjected him to a warm shower bath; then an antimonial emetic was administered, which was followed by a free cathartic of calomel and jalap, or salts and senna, or some other medicine equally active—always taking care to make calomel one of the ingredients. After a free action of the bowels was effected, a solution of tartar emetic was given every few hours, for the purpose either of keeping the patient nauseated, or with the more useful intention of commanding the circulation, and keeping up an action of the skin. When there was much fever, a repetition of the warm shower was directed every few hours, which was among the very best means used for reducing the morbid heat. When these means did not sufficiently control the febrile action, I had recourse again to the lancet, with mercurial and other purgatives, which were given daily. Indeed, at the commencement of this affection, my inclination and opinion led me to a free use of mercury; but as I advanced in the treatment, I became satisfied that salivation had no beneficial influence, and, indeed, that there did not seem to be any advantage in the exhibition of calomel after the proper

action of the abdominal viscera was restored. As there was no ulceration of the fauces, *but a deeply injected condition only*, no gargles were directed but those of a mild character.

To the throat externally, no applications were made, but volatile liniment and emollient poultices. I had long had an abhorrence of blistering in scarlatina, in any form, as I had mostly seen cases where this remedy was used prove fatal, and often, as I thought, from the ill-conditioned state of the blistered surfaces; this was, however, particularly the case in young subjects.

When the disease was mild in its character, my course was very different from the one laid down above: in such cases I did little more than give mild doses of epsom salts, or of oil, with a little calomel, which was given at the onset; and afterwards a light diet was directed with laxatives. I have not unfrequently seen what were obviously mild cases of scarlatina become serious, and end fatally, in consequence of the treatment being so active as to destroy the equilibrium of the circulation, and cause a recession from the surface, with consequent congestions of the brain, abdominal viscera, or other organs, which eventually proved fatal. This result I have occasionally seen after bleeding, when there was much congestion of the skin and subjacent cellular membrane.

Cupping on the temples or back of the neck, had a happy effect in some cases, where there was determination to the brain. I recollect one case in which deep coma existed, that was greatly relieved by cupping. Leeching would no doubt have had a very happy effect; but my location in 1833 prevented any trials being made with it. I did not lose a single case of the simple form of the disease; but the reader knows from the foregoing pages that when a complication with erysipelas took place this fortunate result did not always ensue.

In the treatment of the cases in which erysipelatous inflammation of the face and scalp occurred, but one mode of treatment seemed at all successful; and even that too often failed. The first case of this kind that I met with, was that of a young

lady eighteen years of age. She was of a delicate form, and had a feeble constitution. Her disease had progressed some days before I saw her; and about twenty-four hours anterior, an inflamed belt had crossed the nose, and rested on each cheek. This caused alarm in the minds of her friends, that led to a consultation. I found her nearly pulseless, with delirium and subsultus. Cold applications had been used to the inflamed part; and as yet no vesicatiou had taken place, which had probably been prevented by the application of cold water.

The general treatment of this case had been mercurial. Large and repeated doses of calomel had been given, and but little else. Salivation, however, had not supervened. I directed but little to be done, as I saw that death must take place in a few hours.

I was very soon called to another case, which had been managed by the same medical gentleman, and in which the treatment had been similar to that pursued in the above. Calomel had been given largely, notwithstanding the whole face and scalp were involved in the inflammation, which had produced vesication. A tcndency to coma and delirium was present. I directed a continuance of the calomel, and applied the mercurial ointment to the vesicated surface. I also wished to bleed the patient, but she would not submit. I gave nauseating doses of antimony, and continued them as long as they could be useful. She however succumbed in a few days.

These cases produced a degree of alarm in my mind, as I felt that in them was a masked form of scarlatina, associated with erysipelas that was calculated to set at defiance the best directed efforts. On mature deliberation, however, I determined on the following course: in the first place, to bleed, in all cases where the patient would bear it, and to repeat it as often as was admissible; in the second place, to purge with calomel and jalap, or senna and salts; and in the third place, to vomit with tartar emetic, either immediately before purging, or within a few hours after. The emetics and cathartics to be repeated according to circumstances; and

throughout, antimonials to be given in less than nauseating doses for the purpose of controlling the circulation; whilst blue ointment and poultice should be applied to the blistered surfaces.

This plan I pursued as long as the powers of the system would permit. At this distance of time, I can find but little to regret in my course, unless it be as regards the use of mercurials; for although I occasionally salivated my patients, I am not of the opinion that I saved a single case by it; and if time and reflection can render the judgment more correct, I can now aver that a treatment independent of mercurials would be equally as safe as the contrary. I believe that bleeding, emetics and purgatives, with the constant and untiring use of antimonials from beginning to end, with proper dressings to the vesicated surfaces, were all that were materially useful. To combine calomel with purgatives was possibly useful, but the specific action of mercury seemed to do no good. So far as regards the influence of antimonials in this affection, in preventing a fatal termination, the following case, I presume, will be instructive.

A young lady was taken with scarlatina anginosa, and when I first saw her the inflammation had travelled up the septum narium, and contrary to its usual course appeared at the upper border of the lip, but was rapidly reaching the ridge of the nose. She was bled, and vomited by tartar, which immediately stopped the progress of the inflammation; and it was kept stationary, and eventually removed by the frequent exhibition of nauseating doses of tartar.

Purging will always be resorted to, in those diseases affecting the mucous membrane of the fauces, mouth and nose, because experience has taught most practitioners, that a depletory course (pursued with care) of the stomach and bowels by cathartics has a most happy effect in subduing inflammatory conditions of those parts; hence it is that purgatives of the hydragogue kind are best; such as jalap, epsom salts, &c. When diarrhœa accompanies any form of scarlatina, danger may be apprehended, and Dover's powder, with calomel or

blue mass, will be proper. If the head be not affected, other means must be resorted to. In the disease under consideration there was scarcely a case of diarrhœa that came under my observation.

In order to give a just idea of the beneficial effects of bleeding, I will give a short history of two cases; as I think, both were happily terminated in consequence of repeated bleedings, I shall omit the recital of other cases that would equally prove the efficacy of the free use of the lancet.

A gentleman aged 45 was taken with the usual symptoms of the disease, but had no medical attendance for some days, and by various imprudences, erysipelas of the face was established in the usual way. The inflammation involved the whole face and scalp; articulation and swallowing were almost impossible for days; blindness also was produced by the swelling; abscesses formed in the upper eye-lids, and a number on various points of the scalp, each of which when opened contained a tea-spoonful of pus. Vesication was very general and the discharge profuse. Accompanying this condition was pain in the head, general fever, and almost constant delirium.

This patient was bled from the arm three times, about 30 ounces of blood being taken each time, which had a most happy effect; each successive venesection moderating the reaction, and abating the head-ache and delirium. To this remedy was added an antimonial emetic, with continued nauseating doses, and cathartics of calomel and jalap, with dressings to the inflamed surfaces of mercurial ointment and poultices. This patient became convalescent in about ten days from the commencement of the erysipelatous inflammation, and finally recovered. I will cite one case more.

Very near the conclusion of the epidemic, a youth, aged sixteen, complained a little in the evenlng, but went to bed in the usual way. He arose in the night, stepped over the *head* of his bed, and fell about 10 feet. The side of his occiput struck the edge of the lowest step of the stairs, lacerating the

soft parts to the bone, and producing a profuse hæmorrhage from the occipital artery.

The patient was instantly thrown into a comatose condition from which he did not recover for five days. During this time I scarcely suspected the presence of any disease other than that produced by the fall; and accordingly treated my patient altogether in a surgical point of light. He was bled twice a day for the first five days; and cathartics and antimonials were administered. The wound was treated in the usual way and cold applications were mostly kept to the scalp.

About the time the comatose condition began to subside, I discovered that erysipelas of the nose was setting in, and I now for the first time found the reason why I had had so much difficulty in controlling the violence of what I had regarded as reaction. This young man had not to suffer the same amount of erysipelatous inflammation that others had, and very soon recovered, having lost about six pounds of blood during the first five days after his fall.

With regard to the treatment pursued in those cases where erysipelatous inflammation of the extremities supervened, but little need be said, as a method was adopted very similar to that pursued in the management of the affections of the head. As a number of cases lingered long, it was a matter of necessity to treat them mildly; and in these cases I tried salivation, but no benefit was derived from it. When suppuration took place the abscesses were opened with the knife, and the wound was dressed in the usual way. Poultices were generally applied to the inflamed parts, and sometimes spirits and water with mercurial ointment.

The pregnant patients were treated actively, particularly as regarded the use of the lancet; for I found that free bleeding relieved pain and of course soothed the last moments of those who sunk beneath the weight of this terrible malady. The single case that recovered was managed without any active treatment. Rest was enjoined, a low diet, and mild purgatives in small doses, with minute portions of tartrate of antimony.

During the prevalence of this disease, I lost in St. Clairsville eight patients by it, and in the country six. The whole number treated by me individually was two hundred, of which one hundred were in town. I saw, however, a considerable number of cases in consultation. I believe that no more than fourteen died in town during the epidemic; all of whom, except one, were adults.

Cincinnati, Oct. 1842.

REVIEWS.

Art. IV.—*Animal Chemistry; or Organic Chemistry in its application to Physiology and Pathology.* By Justus Liebig, M. D., Professor of Chemistry in the University of Giessen. Edited from the Author's MS., by William Gregory, M. D., Professor of Chemistry, King's College, Aberdeen. With additions, notes, and corrections, by John W. Webster, M. D., Erving Professor of Chemistry in Harvard University. 1842.

Some one has termed Animal Chemistry the border territory lying between the domains of chemistry and physiology, which has been alternately claimed and occupied by both. Like all other border territories, the limits of which are not well defined, it has formed the theatre for warm and protracted controversies, each party claiming the right of governing it by his own laws. To the chemist, the processes of life have offered nothing more than examples of chemical affinity. In the stomach he has recognised a sort of fermenting vat, and in the lungs a furnace, where fuel is consumed and heat disengaged for warming the body. Some enthusiasts in this profession have not been able to see in all the mysterious functions of living beings any thing higher than a complicated and well arranged chemical laboratory, where composition and resolution, the generation of acids and alkalies, and the development of brain, blood-vessel and nerve go on under the direction of laws which govern the reactions of inorganic matter. The vitalist, on the other hand, has claimed for every action of the living organism the exclusive agency of the vital principle—rejecting all chemical influences as extraneous and hostile—asserting that chemistry and vitality are antagonist principles, each struggling for the ascendency—the vital force prevailing

while the being lives, and only yielding up the body to chemistry when it dies. As in most controversies, truth, we apprehend, will be found to lie between the extremes of these opinions. The theories of the old chemists concerning the functions of vitality, it must be admitted, were visionary and absurd enough; but equally irrational must appear any attempt, at the present day, to solve many of the problems of vitality without a reference to chemistry. For the last twenty-five years physiology has been deriving the most important aid from the researches of the laboratory. Chemistry has improved its principles of research and greatly multiplied its facts, but never before did it receive so rich a contribution from this quarter as in the labors of Professor Liebig, from which we have no doubt a new era will be dated in the advance of physiology. In the work which stands at the head of this article the points of intersection of chemistry with physiology are clearly indicated, and it is shown that these sciences maintain towards each other relations similar to those which exist between chemistry and physics. In no work on animal chemistry has an attempt been made to explain so many vital processes upon chemical principles, and we are perfectly safe in asserting that no work has lately been given to the public, abounding in so many novel and original views, or which has excited in all quarters so startling an interest. When it was in progress it is said to have been submitted by its author to Müller, Tiedmann and Wagner, the three most distinguished physiologists in Germany, and while it is not pretended that they are pledged to the opinions of Professor Liebig, it is stated on high authority that there is but one feeling among them as to the vast importance of chemistry to physiology. We cannot, therefore, help regarding this ancient war as pretty nearly at an end. The physiologist and chemist, as their sciences in many parts are mixed up together, must go hand in hand in the investigation of the phenomena of life, for it is only by their joint labors that we can hope to attain a new physiology and a more rational pathology. Organic chemistry offers to the physiologist a sounding line which, al-

though it is not long enough to measure the depths of the sea he is exploring, is yet the only instrument which can guide him in many of his investigations. The work before us, then, by far the ablest on that subject that has yet appeared, it will readily be believed, is full of interest for the medical man, and with that conviction we have undertaken to give in this article an analysis of its remarkable doctrines.

We will premise, that Dr. Liebig admits the existence of a vital principle, a *vis vitæ*, as distinct from electricity or chemical affinity, which is the source, in animals and vegetables, under the action of external influences, of growth and of reproduction, the subjects to which the first part of his work is devoted.

"We know not," he says, "how a certain something, invisible and imponderable in itself (heat), gives to certain bodies the power of exerting an enormous pressure on surrounding objects; we know not even how this something is produced when we burn wood or coals.

"So it is with the vital force, and with the phenomena exhibited by living bodies. The cause of these phenomena is not chemical force; it is not electricity, nor magnetism; it is a force which has certain properties in common with all causes of motion, and of change in form and structure in all material substances. It is a peculiar force, because it exhibits manifestations which are found in no other known force.

"As in the closed galvanic circuit, in consequence of certain changes which an inorganic body, a metal, undergoes when placed in contact with an acid, a certain something becomes cognizable by our senses, which we call a current of electricity; so, in the animal body, in consequence of transformations and changes undergone by matter previously constituting a part of the organism, certain phenomena of motion and activity are perceived, and these we call life or vitality."

Every motion or manifestation of force, according to our author, is the result of a transformation of the structure or substance of organs; every conception and mental affection is followed by changes in the chemical nature of the secreted fluids, and every thought and every sensation is accompanied by a change in the composition of the substance of the brain. The first conditions of animal life are nutritious matters and

oxygen introduced into the system; and all vital activity arises from the reciprocal action of the oxygen and the elements of the food. At every moment of his life man is taking oxygen into his system from the atmosphere, in a quantity to amount, according to Lavoisier, to 746 lbs., and according to Menzies, to 837 lbs. in one year; and yet, if he has been supplied with sufficient food, we find his weight at the beginning and end of the year, if not quite the same, differing at most by only a few pounds. The question naturally occurs, what has become of the enormous weight of oxygen thus introduced in the course of a year into the human system?

"This question may be answered satisfactorily; no part of this oxygen remains in the system; but it is given out again in the form of a compound of carbon and hydrogen.

"The carbon and hydrogen of certain parts of the body have entered into combination with the oxygen introduced through the lungs and through the skin, and have been given out in the forms of carbonic acid gas and the vapor of water.

"At every moment, with every expiration, certain quantities of its elements separate from the animal organism, after having entered into combination, within the body, with the oxygen of the atmosphere.

"If we assume, with Lavoisier and Séguin, in order to obtain a foundation for our calculation, that an adult may receive into his system daily 32½ oz. (46,037 cubic inches=15,661 grains, French weight) of oxygen, and that the weight of the whole mass of his blood, of which 80 per cent. is water, is 24 lbs.; it then appears, from the known composition of the blood, that, in order to convert the whole of its carbon and hydrogen into carbonic acid and water, 64,103 grains of oxygen are required. This quantity will be taken into the system of an adult in four days two hours.

"Whether this oxygen enters into combination with the elements of the blood, or with other other parts of the body containing carbon and hydrogen, in either case the conclusion is inevitable, that the body of a man, who daily takes into his system 32½ oz. of oxygen, must receive daily in the shape of nourishment, as much carbon and hydrogen as would suffice to supply 24 lbs. of blood with these elements; it being presupposed that the weight of the body remains unchanged, and that it retains its normal condition as to health."

This supply is furnished in the food, which must always be in a direct ratio to the quantity of oxygen taken into the system. Two animals respiring unequal quantities of oxygen will not require the same amount of nourishment; nor will an animal in a state of rest consume as much food as during exercise or work, the number of respirations in the latter case being greater than in the former. A child, whose respiration is more active than that of the adult, requires food oftener and is less patient of hunger. A bird, deprived of food, will die on the third day, while a reptile with its sluggish respiration will go half a year without food. Health suffers, whether the supply of food in proportion to the oxygen respired, be excessive or deficient. In one case obesity would be the result; and in the other dyspepsia and emaciation.

But the quantity of oxygen inspired is affected not only by the activity of the respiratory function, but also by the temperature and density of the atmosphere. In cold weather, and in cold climates, more air, and consequently more oxygen, enters the chest of a man than is respired by him in summer and in southern latitudes; and it is obvious that in an equal number of respirations we consume more oxygen at the level of the sea than on a mountain. The character and quantity of our food ought to correspond to these differences. In winter and in northern latitudes, more food is necessary, and food richer in carbon, upon which the oxygen acts. Certain savages, exposed almost naked to the severity of an arctic winter, will consume ten pounds of flesh, and "a dozen of tallow candles into the bargain," daily—facts which warmly clad travellers speak of with astonishment.

"Even when we consume equal weights of food in cold and warm countries, infinite wisdom has so arranged, that the articles of food in different climates are most unequal in the proportion of carbon they contain. The fruits on which the natives of the south prefer to feed do not in the fresh state contain more than 12 per cent. of carbon, while the bacon and train oil used by the inhabitants of the arctic regions contain from 66 to 80 per cent. of carbon.

"It is no difficult matter in warm climates, to study mode-

ration in eating, and men can bear hunger for a long time under the equator; but cold and hunger united very soon exhaust the body.

"The mutual action between the elements of the food and the oxygen conveyed by the circulation of the blood to every part of the body is THE SOURCE OF ANIMAL HEAT."

It is well known that all animals possess within themselves a source of heat independent of surrounding objects. Even those animals called cold-blooded, maintain a temperature higher than the medium in which they live.

"This high temperature of the animal body, or, as it may be called, disengagement of heat, is uniformly and under all circumstances the result of the combination of a combustible substance with oxygen.

"In whatever way carbon may combine with oxygen, the act of combination cannot take place without the disengagement of heat. It is a matter of indifference whether the combination take place rapidly or slowly, at a high or at a low temperature; the amount of heat liberated is a constant quantity.

"The carbon of the food, which is converted into carbonic acid within the body, must give out exactly as much heat as if it had been directly burnt in the air or in oxygen gas; the only difference is, that the amount of heat produced is diffused over unequal times. In oxygen, the combustion is more rapid, and the heat more intense; in air it is slower, the temperature is not so high, but it continues longer.

"It is obvious, that the amount of heat liberated must increase or diminish with the quantity of oxygen introduced in equal times by respiration. Those animals which respire frequently, and consequently consume much oxygen, possess a higher temperature than others, which, with a body of equal size to be heated, take into the system less oxygen. The temperature of a child (102°) is higher than that of an adult (99.5°.) That of birds (104° to 105.4°) is higher than that of quadrupeds (98.5° to 100.4°) or than that of fishes or amphibia, whose proper temperature is from 2.7° to 3.6° higher than that of the medium in which they live. All animals, strictly speaking, are warm-blooded; but in those only which possess lungs is the temperature of the body quite independent of the surrounding medium."

The temperature of man is the same in all latitudes; yet the loss of heat at Palermo, where the external temperature

is nearly equal to that of the body, and in the polar regions, where the temperature of the air is from 70° to 90° lower, must be very unequal, and the heat given off under the latter circumstances, must be restored within the body with great rapidity. This restoration is effected by the mutual action of the elements of the food and the inspired oxygen, which can never unite without the evolution of heat.

"In the animal body the food is the fuel; with a proper supply of oxygen we obtain the heat given out during its oxidation or combustion. In winter, when we take exercise in a cold atmosphere, and when consequently the amount of inspired oxygen increases, the necessity for food containing carbon and hydrogen increases in the same ratio; and by gratifying the appetite thus excited, we obtain the most efficient protection against the most piercing cold. A starving man is soon frozen to death: and every one knows that the animals of prey in the arctic regions far exceed in voracity those of the torrid zone.

"In cold and temperate climates, the air, which incessantly strives to consume the body, urges man to laborious efforts in order to furnish the means of resistance to its action, while, in hot climates, the necessity of labor to provide food is far less urgent.

"Our clothing is merely an equivalent for a certain amount of food. The more warmly we are clothed the less urgent becomes the appetite for food, because the loss of heat by cooling, and consequently the amount of heat to be supplied by the food, is diminished."

According to this view of the case, the quantity of food required by the system bears an exact ratio to the number of respirations, to the temperature of the air, and to the amount of heat given off to the surrounding medium. On this point Professor Liebig makes the following very interesting remarks:

"The Englishman in Jamaica sees with regret the disappearance of his appetite, previously a source of frequently recurring enjoyment; and he succeeds by the use of cayenne pepper and the most powerful stimulants, in enabling himself to take as much food as he was accustomed to eat at home. But the whole of the carbon thus introduced into the system is not consumed; the temperature of the air is too high, and the oppressive heat does not allow him to increase the number

of respirations by active exercise, and thus to proportion the waste to the amount of food taken; disease of some kind, therefore, ensues.

"On the other hand, England sends her sick, whose diseased digestive organs have in a greater or less degree lost the power of bringing the food into that state in which it is best adapted for oxidation, and therefore furnish less resistance to the oxidizing agency of the atmosphere than is required in their native climate, to southern regions, where the amount of inspired oxygen is diminished in so great a proportion; and the result, an improvement in the health, is obvious. The diseased organs of digestion have sufficient power to place the diminished amount of food in equilibrium with the inspired oxygen; in the colder climate, the organs of respiration themselves would have been consumed in furnishing the necessary resistance to the action of the atmospheric oxygen.

"In our climate, hepatic diseases, or those arising from excess of carbon, prevail in summer; in winter, pulmonic diseases, or those arising from excess of oxygen, are more frequent."

The practical lesson to be drawn from this doctrine is obvious and impressive. Dr. Liebg has afforded a satisfactory explanation of a fact which observation has fully established, and by which men ought to have been taught long ago to adapt their diet to the circumstances of the climate in which they live.

Carbon has been specially alluded to as the element which, by uniting with oxygen, supports the temperature of the animal body, but we are not to lose sight of the fact, that the hydrogen of the food plays a part not less important than the carbon. The whole process of respiration, Professor Liebig remarks, is most clearly developed by the state of an animal wholly deprived of food.

"The first effect of starvation is the disappearance of fat, and this fat cannot be traced either in the urine or in the scanty fæces. Its carbon and hydrogen have been given off through the skin and lungs in the form of oxidized products; it is obvious that they have served to support respiration.

"In the case of a starving man, 32½ oz. of oxygen enter the system daily, and are given out again in combination with a part of his body. Currie mentions the case of an individual

who was unable to swallow, and whose body lost 100 lbs. in weight during a month; and according to Martell (Trans. Linn. Soc., vol. xi. p. 411,) a fat pig, overwhelmed in a slip of earth, lived 160 days without food, and was found to have diminished in weight, in that time, more than 120 lbs. The whole history of hybernating animals, and the well established facts of the periodical accumulation, in various animals, of fat, which, at other periods, entirely disappears, prove that the oxygen, in the respiratory process, consumes, without exception, all such substances as are capable of entering into combination with it. It combines with whatever is presented to it; and the deficiency of hydrogen is the only reason why carbonic acid is the chief product; for, at the temperature of the body, the affinity of hydrogen for oxygen far surpasses that of carbon for the same element."

"In the progress of starvation, however, it is not only the fat which disappears, but also, by degrees, all such of the solids as are capable of being dissolved. In the wasted bodies of those who have suffered starvation, the muscles are shrunk and unnaturally soft, and have lost their contractility; all those parts of the body which were capable of entering into the state of motion, have served to protect the remainder of the frame from the destructive influence of the atmosphere. Toward the end, the particles of the brain begin to undergo the process of oxidation, and delirium, mania, and death close the scene; that is to say, all resistance to the oxidizing power of the atmospheric oxygen ceases, and the chemical process of eremacausis, or decay, commences, in which every part of the body, the bones excepted, enters into combination with oxygen.

"The time which is required to cause death by starvation depends on the amount of fat in the body, on the degree of exercise, as in labor or exertion of any kind, on the temperature of the air, and finally, on the presence or absence of water. Through the skin and lungs there escapes a certain quantity of water, and as the presence of water is essential to the continuance of the vital motions, its dissipation hastens death. Cases have occurred, in which a full supply of water being accessible to the sufferer, death has not occurred, till after the lapse of twenty days. In one case, life was sustained in this way for the period of sixty days.

"In all chronic diseases death is produced by the same cause, namely, the chemical action of the atmosphere. When those substances are wanting, whose function in the organism is to support the process of respiration; when the diseased

organs are incapable of performing their proper function of producing these substances; when they have lost the power of transforming the food into that shape in which it may, by entering into combination with the oxygen of the air, protect the system from its influence, then, the substance of the organs themselves, the fat of the body, the substance of the muscles, the nerves, and the brain, are unavoidably consumed.*

"The true cause of death in these cases is the respiratory process, that is, the action of the atmosphere."

Professor Liebig next proceeds to examine the theory which ascribes to the nervous system the generation of animal heat. He remarks:

"No one will seriously deny the share which the nervous apparatus has in the respiratory process; for no change of condition can occur in the body without the nerves; they are essential to all vital motions. Under their influence, the viscera produce those compounds, which, while they protect the organism from the action of the oxygen of the atmosphere, give rise to animal heat, and when the nerves cease to perform their functions, the whole process of the action of oxygen must assume another form. When the pons Varolii is cut through in the dog, or when a stunning blow is inflicted on the back of the head, the animal continues to respire for some time, often more rapidly than in the normal state; the frequency of the pulse at first rather increases than diminishes, yet the animal cools as rapidly as if sudden death had occurred. Exactly similar observations have been made on the cutting of the spinal cord, and of the par vagum. The respiratory motions continue for a time, but the oxygen does not meet with those substances with which, in the normal state, it would have combined; because the paralyzed viscera will no longer furnish them. The singular idea that the nerves produce animal heat, has obviously arisen from the notion that the inspired oxygen combines with carbon, in the blood itself; in which case the temperature of the body, in the above experiments, certainly, ought not to have sunk. But, as we shall afterwards see, there cannot be a more erroneous conception than this.

"As by the division of the pneumogastric nerves the motion of the stomach and the secretion of the gastric juice are arrest-

*For an account of what really takes place in this process, I refer to the considerations on the means by which the change of matter is effected in the body of the carnivora, which will be found further on.

ed, and an immediate check is thus given to the process of digestion, so the paralysis of the organs of vital motion in the abdominal viscera affects the process of respiration. These processes are most intimately connected; and every disturbance of the nervous system or of the nerves of digestion reacts visibly on the process of respiration."

Our author proves by the most indisputable calculations, that the heat evolved during the union of oxygen with the elements of the food is quite sufficient to maintain the temperature of the animal body. Why, then, look further for other mysterious sources of caloric?

"According to the experiments of Despretz, 1 oz. of carbon evolves, during its combustion, as much heat as would raise the temperature of 78.15 oz. of water at 32° to 212°, that is, by 180 degrees; in all, therefore, 78.15 times 180°=14067 degrees of heat. Consequently, the 13.9 oz. of carbon which are daily converted into carbonic acid in the body of an adult, evolve 13.9+14067°=195531.3 degrees of heat. This amount of heat is sufficient to raise the temperature of 1 oz. of water by that number of degrees, or from 32° to 195563.3°; or to cause 67.9 lbs. of water at 32° to boil; or to heat 184 3 lbs. of water to 98.3° (the temperature of the human body;) or to convert into vapor 11.4 lbs. of water at 98.3°.

"If we now assume that the quantity of water vaporized through the skin and lungs in 24 hours amounts to 48 oz. (3 lbs.,) then there will remain, after deducting the necessary amount of heat, 144137.7 degrees of heat, which are dissipated by radiation, by heating the expired air, and in the excrementitious matters.

"In this calculation, no account has been taken of the heat evolved by the hydrogen of the food, during its conversion into water by oxidation within the body. But if we consider that the specific heat of the bones, of fat, and of the organs generally, is far less than that of water, and that consequently they require, in order to be heated to 98.3°, much less heat than an equal weight of water, no doubt can be entertained, that when all the concomitant circumstances are included in the calculation, the heat evolved in the process of combustion, to which the food is subjected in the body, is amply sufficient to explain the constant temperature of the body, as well as the evaporation from the skin and lungs."

This explanation of the source of animal temperature is not peculiar to Dr. Liebig, but he has presented the doctrine

in the clearest light, and supported it by an array of facts and arguments which appear to us to have settled the question.

On the subject of *reproduction* the author throws out the following hints, which are so curious that we are tempted to quote them:

"In a young animal the waste is less than the increase; and the female retains, up to a certain age, this peculiar condition of a more intense vegetative life. This condition does not cease in the female as in the male, with the complete development of all the organs of the body.

"The female in the lower animals, is, at certain seasons, capable of reproduction of the species. The vegetative life in her organism is rendered more intense by certain external conditions, such as temperature, food, &c.; the organism produces more than is wasted, and the result is the capacity of reproduction.

"In the human species, the female organism is independent of those external causes which increase the intensity of vegetative life. When the organism is fully developed, it is at all times capable of reproduction of the species; and infinite wisdom has given to the female body the power, up to a certain age, of producing all parts of its organization in greater quantity than is required to supply the daily waste.

"This excess of production can be shown to contain all the elements of a new organism; it is constantly accumulating, and is periodically expelled from the body until it is expended in reproduction. This periodical discharge ceases when the ovum has been impregnated, and from this time every drop of the superabundant blood goes to produce an organism like that of the mother.

"Exercise and labor cause a diminution in the quantity of the menstrual discharge; and when it is suppressed in consequence of disease, the vegetative life is manifested in a morbid production of fat. When the equilibrium between the vegetative and nervous life is disturbed in the male, when, as in eunuchs, the intensity of the latter is diminished, the predominance of the former is shown in the same form, in an increased deposit of fat."

The blood supplies the materials for the growth of the system and for repairing its waste. No substance, therefore, can be considered nutritious which is not susceptible of conversion into blood. The two most important ingredients of

the blood, the fibrine and albumen, have very unexpectedly been proved to be identical in composition. They contain the elements out of which the living frame is built up—nitrogen, phosphorus, sulphur, the earth of bones, sea salt and other salts of soda and potash, carbonic, phosphoric and sulphuric acids. The red globules with fibrine and albumen contain iron as a constant element. Fatty bodies, of a peculiar kind, are also found in the blood, and water forms its largest ingredient.

"Both the albumen and fibrine, in the process of nutrition, are capable of being converted into muscular fibre, and muscular fibre is capable of being reconverted into blood. These facts have long been established by physiologists, and chemistry has merely proved that these metamorphoses can be accomplished under the influence of a certain force, without the aid of a third substance, or of its elements, and without the addition of any foreign element, or the separation of any element previously present in these substances.

"If we now compare the composition of all organized parts with that of fibrine and albumen, the following relations present themselves—

"All parts of the animal body which have a decided shape, which form parts of organs, contain nitrogen. No part of an organ which possesses motion and life, is destitute of nitrogen; all of them contain likewise carbon and the elements of water, the latter, however, in no case in the proportion to form water.

"The chief ingredients of the blood contain nearly 17 per cent. of nitrogen, and no part of an organ contains less than 17 per cent. of nitrogen.

"The most convincing experiments and observations have proved that the animal body is absolutely incapable of producing an elementary body, such as carbon or nitrogen, out of substances which do not contain it; and it obviously follows, that all kinds of food fit for the production either of blood, or of cellular tissue, membranes, skin, hair, muscular fibre, &c., must contain a certain amount of nitrogen, because that element is essential to the composition of the above named organs; because the organs cannot create it from the other elements presented to them; and, finally, because no nitrogen is absorbed from the atmosphere in the vital process.

"The substance of the brain and nerves contains a large quantity of albumen, and, in addition to this, two peculiar

fatty acids, distinguished from other fats by containing phosphorus (phosphoric acid?) One of these contains nitrogen (Frémy)."

In carnivorous animals the process of nutrition is seen in its simplest form. They live upon substances identical with their own flesh and blood, and in a chemical sense it may be said that, in supporting the vital process, they consume themselves. All their food is derived from blood, and being dissolved in the stomach, it is again, in its passage through the system, converted into blood, from which is reproduced all those parts of their organization which have undergone change or metamorphosis.

"The process of nutrition in graminivorous animals appears at first sight altogether different. The digestive organs are less simple, and their food consists of vegetables, the great mass of which contains but little nitrogen.

"From what substances, it may be asked, is the blood formed, by means of which their organs are developed? This question may be answered with certainty.

"Chemical researches have shown, that all such parts of vegetables as can afford nutriment to animals contain certain constituents which are rich in nitrogen; and the most ordinary experience proves that animals require for their support and nutrition less of these parts of plants in proportion as they abound in the nitrogenized constituents. Animals cannot be fed on matters destitute of these nitrogenized constituents.

These important products of vegetation are especially abundant in the seeds of the different kinds of grain, and of pease, beans and lentils; in the roots and the juices of what are commonly called vegetables. They exist, however, in all plants, without exception, and in every part of plants in larger or smaller quantity,"

These substances are *vegetable fibrine, vegetable albumen,* and *vegetable caseine,* nitrogonized compounds, out of which the blood of the herbivora is formed. Professor Liebig remarks:

"The chemical analysis of these three substances has led to the very interesting result that they contain the same organic elements, united in the same proportion by weight; and, what is still more remarkable, that they are identical in composition with the chief constituents of blood, animal fibrine, and

albumen. They all three dissolve in concentrated muriatic acid with the same deep purple color, and even in their physical characters, animal fibrine and albumen are in no sense different from vegetable fibrine and albumen. It is especially to be noticed, that by the phrase identity of composition, we do not here imply mere similarity, but that even in regard to the presence and relative amount of sulphur, phosphorus, and phosphate of lime, no difference can be observed."

Vegetables, therefore, produce the blood of animals, for the carnivora, in consuming the graminivora, derive only such vegetable principles as had served for their nourishment. The vegetable fibrine and albumen which made the blood of the graminivorous animals, enter into the system of the carnivorous animal to constitute his flesh and blood. The development of the animal organism can only take place by the reception of certain principles identical with the chief ingredients of the blood. The blood it cannot create, but only receives it from substances which contain its chief constituents and gives to it its peculiar form. Out of the blood it produces nervous matter, cellular tissue, muscle, tendon and the various other parts which make up the animal frame, and which vegetables cannot produce.

But, besides these elements of food, rich in nitrogen, which contribute chiefly to the growth of animals, there are found other substances, as sugar, starch, and gum, necessary to the support of the animal system. The herbivorous animals quickly perish unless these compounds be supplied. What is the function performed by them? According to Professor Liebig, they cannot be employed in the production of blood, because the azotized substances of the food contain exactly the amount of carbon required for the formation of fibrine and albumen, and they must, therefore, be expended in the production of animal heat. The young of carnivorous animals find the requisite supply of nitrogenized food in the caseine of the the milk, which, curiously enough, turns out on analysis to be identical with the chief constituents of blood, fibrine and albumen—nay more, to be identical in all its properties with vegetable caseine. The caseine is converted into blood without

the agency of any foreign substance, and the butter and the sugar of milk, containing no nitrogen, and only carbon, hydrogen and oxygen, become fuel for the maintenance of animal heat. The author proceeds:

"In the preceding pages it has been assumed, that the elements of the food are converted by the oxygen absorbed in the lungs into oxidized products; the carbon into carbonic acid, the hydrogen into water, and the nitrogen into a compound containing the same elements as carbonate of ammonia.

"This is only true in appearance; the body, no doubt, after a certain time, acquires its original weight. The amount of carbon, and of the other elements, is not found to be increased —exactly as much carbon, hydrogen, and nitrogen has been given out as was supplied in their food; but nothing is more certain than that the carbon, hydrogen, and nitrogen given out, although equal in amount to what is supplied in that form, do not directly proceed from the food.

"It would be utterly irrational to suppose that the necessity of taking food, or the satisfying the appetite, had no other object than the production of urea, uric acid, carbonic acid, and other excrementitious matters—of substances which the system expels, and consequently applies to no useful purposes in the economy.

"In the adult animal, the food serves to restore the waste of matter; certain parts of its organs have lost the state of vitality, have been expelled from the substance of the organs, and have been metamorphosed into new combinations, which are amorphous and unorganized.

"The food of the carnivora is at once converted into blood; out of the newly-formed blood those parts of organs which have undergone metamorphoses are reproduced. The carbon and nitrogen of the food thus become constituent parts of organs.

"Exactly as much carbon and nitrogen is supplied to the organs by the blood, that is, ultimately, by the food, as they have lost by the transformation attending the exercise of their functions.

"What then, it may be asked, becomes of the new compounds produced by the transformations of the organs, of the muscles, of the membranes and cellular tissue, of the nerves and brain?"

These compounds cannot be employed for the reproduction

of those tissues from which they are derived, and if allowed to accumulate in the veins and lymphatics, they would speedily put a stop to the nutritive process. They are removed by an admirable contrivance thus described by Professor Liebig:

"The venous blood, before reaching the heart, is made to pass through the liver; the arterial blood, on the other hand, passes through the kidneys; and these organs separate from both all substances incapable of contributing to nutrition.

"Those new compounds wich contain the nitrogen of the transformed organs are collected in the urinary bladder, and being utterly incapable of any further application in the system, are expelled from the body.

"Those, again, which contain the carbon of the transformed tissues, are collected in the gall-bladder in the form of a compound of soda, *the bile*, which is miscible with water in every proportion, and which, passing into the duodenum, mixes with chyme. All those parts of the bile which, during the digestive process, do not lose their solubility, return during that process into the circulation in a state of extreme division. The soda of the bile, and those highly carbonized portions which are not precipitated by a weak acid (together making 99-100ths of the solid contents of the bile), retain the capacity of resorption by the small and large intestines; nay, this capacity has been directly proved by the administration of enemata containing bile, the whole of the bile disappearing with the injected fluid in the rectum.

"Thus we know with certainty, that the nitrogenized compounds, produced by the metamorphosis of organized tissues, after being separated from the arterial blood by means of the kidneys, are expelled from the body as utterly incapable of further alteration; while the compounds rich in carbon, derived from the same source, return into the system of carnivorous animals.

"The food of the carnivora is identical with the chief constituents of their bodies, and hence the metamorphoses which their organs undergo must be the same as those which, under the influence of the vital force, take place in the matters which constitute their food.

"The flesh and blood consumed as food yield their carbon for the support af the respiratory process, while its nitrogen appears as uric acid, ammonia, or urea. But previously to these final changes, the dead flesh and blood become living flesh and blood, and it is, strictly speaking, the carbon of the

compounds formed in the metamorphoses of living tissues that serves for the production of animal heat.

"The food of the carnivora is converted into blood, which is destined for the reproduction of organized tissues; and by means of the circulation a current of oxygen is conveyed to every part of the body. The globules of the blood, which in themselves can be shown to take no share in the nutritive process, serve to transport this oxygen, which they give up in their passage through the capillary vessels. Here the current of oxygen meets with the compounds produced by the transformation of the tissues, and combines with their carbon to form carbonic acid, with their hydrogen to form water. Every portion of these substances which escapes this process of oxidation is sent back into the circulation in the form of the bile, which by degrees completely disappears.

"In the carnivora the bile contains the carbon of the metamorphosed tissues; this carbon disappears in the animal body, and the bile likewise disappears in the vital process. Its carbon and hydrogen are given out through the skin and lungs as carbonic acid and water; and hence it is obvious that the elements of the bile serve for respiration and for the production of animal heat. Every part of the food of carnivorous animals is capable of forming blood; their excrements, excluding the urine, contain only inorganic substances, such as phosphate of lime; and the small quantity of organic matter which is found mixed with these is derived from excretions, the use of which is to promote their passage through the intestines, such as mucus. These excrements contain no bile and no soda; for water extracts from them no trace of any substance resembling bile, yet bile is very soluble in water, and mixes with it in every proportion.

"Physiologists can entertain no doubt as to the origin of the constituent parts of the urine and of the bile. When, from deprivation of food, the stomach contracts itself so as to resemble a portion of intestine, the gall-bladder, for want of the motion which the full stomach gives to it, cannot pour out the bile it contains; hence in animals starved to death, we find the gall-bladder distended and full. The secretion of bile and of urine goes on during the winter sleep of hybernating animals; and we know that the urine of dogs, fed for three weeks, exclusively on pure sugar, contains as much of the most highly nitrogenized constituent, urea, as in the normal condition."

Professor Liebig rejects the idea that the bile is intended

merely to be excreted. The following are his observations on this subject:

"Were the bile intended merely for excretion, we should find it more or less altered, and also the soda it contains, in the solid excrements. But, with the exception of common salt and of sulphate of soda, which occur in all the animal fluids, only mere traces of soda are to be found in the fæces. The soda of the bile, therefore, at all events, must have returned from the intestinal canal into the organism, and the same must be true of the organic matters which were in combination with it.

"According to the observations of physiologists, a man secretes daily from 17 to 24 oz. of bile; a large dog, 36 oz.; a horse 37 lbs. (Burdach's Physiologie, V. p. 260.) But the fæces of a man do not on an average weigh more than 5½ oz.; and those of a horse 28½ lbs., of which 21 lbs. are water, and 7½ lbs. dry fæces. (Boussingault.) The latter yield to alcohol only 1-20th part of their weight of soluble matter.

"If we assume the bile to contain 90 per cent. of water, a horse secretes daily 592 oz. of bile, containing 59.2 oz. of solid matter, while 7½ lbs. or 120 oz. of dried excrement yield only 6 oz. of matter soluble in alcohol, which might possibly be bile. But this matter is not bile; when the alcohol is dissipated by evaporation, there remains a soft, unctuous mass, altogether insoluble in water, and which, when incinerated, leaves no alkaline ashes, no soda.

During the digestive process, therefore, the soda of the bile, and, along with it, all the soluble parts of that fluid, are returned into the circulation. This soda reappears in the newly formed blood, and, finally, we find it in the urine in the form of phosphate, carbonate, and hippurate of soda. Berzelius found in 1000 parts of fresh human fæces only 9 parts of a substance similar to bile; 5 ounces, therefore, would contain only 21 grains of dried bile, equivalent to 210 grains of fresh bile. But a man secretes daily from 9,640 to 11,520 grains of fluid bile, that is, from 45 to 56 times as much as can be detected in the matters discharged by the intestinal canal."

And the results at which he arrives are thus stated:

"It cannot be disputed, that in an adult carnivorous animal, which neither gains nor loses weight perceptibly from day to day, its nourishment, the waste of organized tissue, and its consumption of oxygen, stand to each other in a well-defined and fixed relation.

"The carbon of the carbonic acid given off, with that of the urine; the nitrogen of the urine, and the hydrogen given off as ammonia and water; these elements, taken together, must be exactly equal in weight to the carbon, nitrogen, and hydrogen of the metamorphosed tissues; and since these last are exactly replaced by the food, to the carbon, nitrogen, and hydrogen of the food. Were this not the case, the weight of the animal could not possibly remain unchanged."

The fact has been alluded to that herbivorous animals consume matters containing no nitrogen, and destined for other purposes than the production of blood. In this class of animals the metamorphosis of existing tissues goes on much less rapidly than in the carnivora, and hence the necessity for materials of aliment which afford to their systems the elements for uniting with oxygen. Were the metamorphosis as rapid as in the systems of carnivorous animals, pastures a thousand times more luxuriant than the existing ones would scarcely suffice for their nourishment; and in that case sugar, gum, and starch would no longer be necessary to their sustenance, because the products of the waste of the organized tissues would contain enough carbon to support the respiratory function.

"Man, when confined to animal food, requires for his support and nourishment extensive sources of food, even more widely extended than the lion and tiger, because, when he has the opportunity, he kills without eating.

A nation of hunters, on a limited space, is utterly incapable of increasing in its numbers beyond a certain point, which is soon attained. The carbon necessary for respiration must be obtained from the animals, of which only a limited number can live on the space supposed. These animals collect from plants the constituents of their organs and of their blood, and yield them, in turn, to the savages who live by the chase alone. They, again, receive this food unaccompanied by those compounds, destitute of nitrogen, which, during the life of the animals, served to support the respiratory process. In such men, confined to an animal diet, it is the carbon of the flesh and of the blood which must take the place of starch and sugar.

"But 15 lbs. of flesh contain not more carbon than 4 lbs. of starch, and while the savage with one animal and an equal

weight of starch could maintain life and health for a certain number of days, he would be compelled, if confined to flesh, in order to procure the carbon necessary for respiration, during the same time, to consume five such animals.

"It is easy to see, from these considerations, how close the connection is between agriculture and the multiplication of the human species. The cultivation of our crops has ultimately no other object than the production of a maximum of those substances which are adapted for assimilation and respiration, in the smallest possible space. Grain and other nutritious vegetables yield us, not only in starch, sugar, and gum, the carbon which protects our organs from the action of oxygen, and produces in the organism the heat which is essential to life, but is also in the form of vegetable fibrine, albumen, and caseine, our blood, from which the other parts of our body are developed.

"Man, when confined to animal food, respires, like the carnivora, at the expense of the matters produced by the metamorphosis of organized tissues; and, just as the lion, tiger, hyæna, in the cages of a menagerie, are compelled to accelerate the waste of the organized tissues by incessant motion, in order to furnish the matter necessary for respiration, so, the savage, for the very same object is forced to make the most laborious exertions and go through a vast amount of muscular exercise. He is compelled to consume force merely in order to supply matter for respiration."

The metamorphosis of tissues appears in the urine, which shows the waste in the carnivora to be much greater than in the graminivora. It is a novel thought of our author, that the phosphoric acid thus resulting goes to the production of brain in this latter class of animals. His words are,

"In the graminivora, therefore, whose food contains so small a proportion of phosphorus or of phosphates, the organism collects all the soluble phosphates produced by the metamorphosis of tissues, and employs them for the development of the bones and of the phosphorized constituents of the brain; the organs of excretion do not separate these salts from the blood. The phosphoric acid which, by the change of matter, is separated in the uncombined state, is not expelled from the body as phosphate of soda; but we find it in the solid excrements in the form of insoluble earthy phosphates."

In this class of animals the intensity of vegetative life far exceeds that of the carnivora. The cat will eat the first

mouse presented to her, and perhaps the second, but even if she kills a third, she does not devour it. The cow and the horse eat almost without interruption from morning till night, and their systems convert into organized tissues all the food they consume, beyond the quantity required for merely supplying the waste of their bodies. They grow fleshy and plump, while the flesh of the carnivorous animal is always tough and sinewy. In wild animals of the herbivorous class, owing to the great exercise they take, oxygen enough is absorbed by respiration to consume the carbon of the gum, sugar and starch, and they remain comparatively lean. With our domestic animals the case is far different.

"The stall-fed animal eats, and reposes merely for digestion. It devours in the shape of nitrogenized compounds far more food than is required for reproduction, or the supply of waste alone; and at the same time it eats far more of substances devoid of nitrogen than is necessary merely to support respiration and to keep up animal heat. Want of exercise and diminished cooling are equivalent to a deficient supply of oxygen; for when these circumstances occur, the animal absorbs much less oxygen than is required to convert into carbonic acid the carbon of the substances destined for respiration. Only a small part of the excess of carbon thus occasioned is expelled from the body in the horse and ox, in the form of hippuric acid; and all the remainder is employed in the production of a substance, which, in the normal state, only occurs in small quantity as a constituent of the nerves and brain. This substance is *fat*."

Now, it is a most curious fact, as stated by Prof. Liebig, that if we compare the composition of sugar and starch with that of mutton and beef suet and of human fat, we find that in all of them the proportion of carbon to hydrogen is the same, and that they only differ in that of oxygen, so that sugar, starch and gum, by the mere separation of a part of their oxygen, may pass into fat. The herbs consumed by the cow contain no butter, and no lard can be found in the corn and potatoes on which hogs are fatted, but these deposits are created in the organism of the animals, and, if the views of Prof. L. be correct, by the separation of oxygen from the elements with which it is combined in the food of the animals.

"Since the carbon of the fatty constituents of the animal body is derived from the food, seeing that there is no other source whence it can be derived, it is obvious if we suppose fat to be formed from albumen, fibrine, or caseine, that, for every 120 equivalents of carbon deposited as fat, 26 equivalents of oxygen must be separated from the elements of these substances; and further, if we conceive fat to be formed from starch, sugar, or sugar of milk, that for the same amount of carbon there must be separated 90, 100, and 110 equivalents of oxygen from these compounds respectively.

"There is, therefore, but one way in which the formation of fat in the animal body is possible, and this is absolutely the same in which its formation in plants takes place; it is a separation of oxygen from the elements of the food."

But this oxygen is not given out in the free state, because it meets in the organism with substances possessing the property of entering into combination with it. A remarkable connection is thus shown to exist between the formation of fat and the respiratory apparatus.

"The abnormal condition, which causes the deposit of fat in the animal body, depends, as was formerly stated, on the disproportion between the quantity of carbon in the food and that of oxygen absorbed by the skin and lungs. In the normal condition, the quantity of carbon given out is exactly equal to that which is taken in the food, and the body acquires no increase of weight from the accumulation of substances containing much carbon and no nitrogen.

"If we increase the supply of highly carbonized food, then the normal state can only be preserved on the condition that, by exercise and labor the waste of the body is increased, and the supply of oxygen augmented in the same proportion.

"The production of fat is always a consequence of a deficient supply of oxygen, for oxygen is absolutely indispensable for the dissipation of the excess of carbon in the food. This excess of carbon deposited in the form of fat, is never seen in the Bedouin or in the Arab of the desert, who exhibits with pride to the traveller, his lean, muscular, sinewy limbs, altogether free from fat; but in prisons and jails it appears as a puffiness in the inmates, fed, as they are, on a poor and scanty diet; it appears in the sedentary females of oriental countries; and finally, it is produced under the well-known condition of the fattening of domestic animals.

"The formation of fat depends on a deficiency of oxygen; but

inthis process, in the formation of fat itself, there is opened up a new source of oxygen, a new cause of animal heat.

"The oxygen set free in the formation of fat is given out in combination with carbon or hydrogen; and whether this carbon and hydrogen proceed from the substance that yields the oxygen, or from other compounds, still there must have been generated by this formation of carbonic acid or water, as much heat as if an equal weight of carbon or hydrogen had been burned in air or in oxygen gas."

Professor Liebig shows by calculation, that every pound of carbon which obtains the oxygen necessary to convert it into carbonic acid from substances which thereby pass into fat, must disengage as much heat as would raise the temperature of 200° lbs. of water by 70°—that is from 32° to 102°.

"Thus, in the formation of fat, the vital force possesses a means of counteracting a deficiency in the supply of oxygen, and consequently in that of the heat indispensable for the vital process."

"In some diseases, the starch, sugar, &c. of the food obviously do not undergo the changes which enable them to assist in respiration, and consequently to be converted into fat. Thus, in diabetes mellitus, the starch is only converted into grape sugar, which is expelled from the body without further change.

"In other diseases, as for example in inflammation of the liver, we find the blood loaded with fat and oil; and in the composition of the bile there is nothing at all inconsistent with the supposition that some of its constituents may be transformed into fat."

All aliments for man, according to our author, may be divided into two classes—*nitrogenized* and *non-nitrogenized*—the former, capable of conversion into blood, and called *the plastic elements of nutrition*, the latter incapable of this transformation, and which may be styled *elements of respiration*. Among the former he reckons vegetable fibrine, albumen, and caseine, and animal flesh and blood; and among the latter, fat, starch, gum, grape sugar, cane sugar, sugar of milk, pectine, bassorine, wine, beer, and spirits. The nitrogenized materials of food are identical in composition with the constituents of blood, and no nitrogenized substance the composition

of which differs from that of fibrine, albumen and caseine is capable of supporting the vital process in animals. The blood, as has been remarked, must be supplied to them ready formed, and then their organism is prepared to form membranes, nerves, cartilages and the organic part of bones. But a question is suggested here as to the agency of gelatine in nutrition. It is not susceptible of conversion into blood, and it is known that dogs fed exclusively upon it starve, yet the gelatine of bones devoured by them entirely disappears. The same is true of the gelatine of the soup eaten by man, not a particle of which can be detected in any of his excretions. What becomes of it? The author thinks the opinion worthy of investigation, that it is again converted in the body into cellular tissue, membrane and cartilage, and may serve for the reproduction of such parts of these tissues as have been wasted, and for their growth. In the sick man, he remarks, the intensity of the vital force must be diminished, as well in the stomach, as in all other parts of the body. In this condition, it is well known that gelatinous matters, in a dissolved state exercise a beneficial influence on the state of the health. Given in a form adapted for assimilation, they serve to husband the vital force, just as may be done, in the case of the stomach, by due preparation of the food in general. And as the animal organism finds it difficult, if not impossible, to convert gelatine into blood, so the tissues chiefly composed of it, as the skin, cartilages, and membranes, yield slowly to the causes which effect rapid transformations in other parts of the system. While the fat in the body of a sick or starving man quickly disappears, and his muscular tissue is converted once more into blood, the tendons and membranes are but slightly diminished in size or altered in condition.

In part II, Professor Liebig treats of what he terms the *metamorphosis of the tissues*, under which head we find his theory of *digestion*. Albumen, fibrine and caseine, we have seen, are the elements out of which blood, and the other parts of the animal organism are formed, and these products of vegetable life, as has been shown, are identical in composition.

When exposed to a high temperature in a solution of caustic potash, they are decomposed, and by the action of acetic acid a precipitate is thrown down of the same precise character from each of these substances, which, on careful analysis, is found to contain the same organic elements, in exactly the same proportion, as the animal matters from which it is prepared. This product Mulder, to whom we owe the discovery of it, named *proteine*—the commencement and starting point of the animal tissues. The blood, or the constituents of the blood, appear to be compounds of this proteine with variable proportions of inorganic substances. Vegetables produce compounds of proteine, and out of these compounds the various parts of the animal body are developed by the vital force, with the aid of the oxygen of the atmosphere and of the elements of water.

"This proposition must be received as an undeniable truth, when we reflect on the development of the young animal in the egg of a fowl. The egg can be shown to contain no other nitrogenised compound except albumen. The albumen of the yolk is identical with that of the white; the yolk contains, besides, only a yellow fat, in which cholesterine and iron may be detected. Yet we see, in the process of incubation, during which no food and no foreign matter, except the oxygen of the air, is introduced, or can take part in the development of the animal, that out of the albumen, feathers, claws, globules of the blood, fibrine, membrane and cellular tissue, arteries and veins, are produced. The fat of the yolk may have contributed, to a certain extent, to the formation of the nerves and brain; but the carbon of this fat cannot have been employed to produce the organized tissues in which vitality resides, because the albumen of the white and of the yolk already contains, for the quantity of nitrogen present, exactly the proportion of carbon required for the formation of these tissues.

"The true starting-point for all the tissues is, consequently, albumen; all nitrogenized articles of food, whether derived from the animal or from the vegetable kingdom, are converted into albumen before they can take part in the process of nutrition."

Digestion, according to the author, is a chemical action similar to those processes of decomposition or transformation,

known as putrefaction, fermentation, or decay, which latter he expresses by the word *eremacausis.* But it must be borne in mind that these chemical changes are not necessarily attended by the more striking phenomena of putrefaction and fermentation—there is not always present an offensive smell, nor is there of necessity any disengagment of gases. Starch is converted into sugar by the action of sulphuric acid without any escape of gas; fibrine and hard boiled white of an egg are transformed into liquids, by organic acids, or weak alkaline solutions, without the slightest change of properties except the solid form; and so there are numberless cases in which a complete chemical metamorphosis occurs without the smallest disengagement of gaseous matter. Expressed in the simplest form Professor Liebig remarks:

"Fermentation, or putrefaction, may be described as a process of transformation—that is, a new arrangement of the elementary particles, of a compound, yielding two or more new groups or compounds, and caused by contact with other substances, the elementary particles of which are themselves in a state of transformation or decomposition. It is a communication, or an imparting of a state of motion, which the atoms of a body in a state of motion are capable of producing in other bodies, whose elementary particles are held together only by a feeble attraction.

"Thus the clear gastric juice contains a substance in a state of transformation, by the contact of which with those constituents of the food which, by themselves, are insoluble in water, the latter acquire, in virtue of a new grouping of their atoms, the property of dissolving in that fluid. During digestion, the gastric juice, when separated, is found to contain a free mineral acid, the presence of which checks all further change. That the food is rendered soluble quite independently of the vitality of the digestive organs has been proved by a number of the most beautiful experiments. Food, enclosed in perforated metallic tubes, so that it could not come into contact with the stomach, was found to disappear as rapidly, and to be as perfectly digested, as if the covering had been absent; and fresh gastric juice, out of the body, when boiled white of egg, or muscular fibre, were kept in contact with it for a time at the temperature of the body, caused these substances to lose the solid form and to dissolve in the liquid."

"In the action of the gastric juice on the food, no other ele-

ment takes a share, except the oxygen of the atmosphere and the elements of water. This oxygen is introduced directly into the stomach. During the mastication of the food, there is secreted into the mouth from organs specially destined to this function, a fluid, the saliva, which possesses the remarkable property of enclosing air in the shape of froth, in a far higher degree than even soapsuds. This air, by means of the saliva, reaches the stomach with the food, and there its oxygen enters into combination, while its nitrogen is given out through the skin and lungs. The longer digestion continues, that is, the greater resistance offered to the solvent action by the food, the more saliva, and consequently the more air enters the stomach. Rumination, in certain graminivorous animals, has plainly for one object a renewed and repeated introduction of oxygen; for a more minute mechanical division of the food only shortens the time required for solution."

The gastric juice is a product of the transformation of the stomach itself, and as yeast, which is nothing but vegetable fibrine, albumen or caseine in a state of decomposition, converts sugar into alcohol and carbonic acid; or as muscular fibre, when separated from the body, decomposes the peroxide of hydrogen, by virtue of the state of decomposition in which it is, so the gastric juice, the result of transformation imparts to the food a tendency to change—the insoluble matters become soluble, or are digested in consequence of an impulse which the gastric juice imparts. This is Professor Liebig's theory of digestion, and it is certainly at the same time a most simple and ingenious one.

And equally simple, according to this view, are the other transformations which take place in the system. Thus proteine passes into chondrine (the substance of the cartilages of the ribs,) by combining with the elements of water and oxygen; while in the formation of the serous membranes, nitrogen also has entered into combination. In horn, hair, &c., to the proteine, nitrogen and hydrogen are added in proportions to form ammonia. All the tissues of the body contain, for the same amount of carbon, more oxygen than the constituents of the blood, and hence during their formation oxygen, either from the atmosphere, or from the elements of water, must be

added to the elements of proteine. These conclusions are followed by others still more curious.

"If it be true, that all parts of the body are formed and developed from the blood or the constituents of the blood, that the existing organs at every moment of life are transformed into new compounds under the influence of the oxygen introduced in the blood, then the animal secretions must of necessity contain the products of the metamorphosis of the tissues.

"If it be further true, that the urine contains those products of metamorphosis which contain the most nitrogen, and the bile those which are richest in carbon, from all the tissues which in the vital process have been transformed into unorganized compounds, it is clear that the elements of the bile and of the urine, added together, must be equal, in the relative proportion of these elements to the composition of the blood.

"The organs are formed from the blood, and contain the elements of the blood; they become transformed into new compounds, with the addition only of oxygen and water. Hence the relative proportion of carbon and nitrogen must be the same as in blood.

"If then we subtract from the composition of blood the elements of the urine, then the remainder, deducting the oxygen and water which have been added, must give the composition of the bile.

"Or if from the elements of the blood, we subtract the elements of the bile, the remainder must give the composition of urate of ammonia, or of urea and carbonic acid."

The transformed muscular fibre is to be looked for in the bile and urine, and is found, according to our author, in the urea of the latter and the choleic acid of the former. Urea degenerates into uric acid when the supply of oxygen is deficient, and it is a very remarkable fact that urinary calculi in the human subject vary with the amount of exercise taken by the individual, and consequently with the quantity of oxygen consumed. The calculi of oxalate of lime, or urate of ammonia are formed in persons in whom, from want of exercise, the supply of oxygen has been diminished. The calculi of patients who have resided in cities are succeeded by deposits containing more oxygen, when they go to the country; and

finally when the person regains entire health the highest degree of oxidation takes place, and only carbonic acid and urea are formed in the kidneys.

The bile is a highly important product of the animal system, and the view which Professor Liebig takes of it, if it should be established by future researches, will enhance its interest. That element of the bile which contains the compounds resulting from the transformation of proteine, is choleic acid. In the carnivora it comes from the metamorphosed tissues; but not so in herbivorous animals. In them, other substances besides compounds of proteine must take part in the formation of bile, because the metamorphosis of tissues is not sufficient to account for the large quantity of this secretion formed. These substances, our author supposes, are sugar, starch, &c., which, uniting with the nitrogenized elements of food, supply the blood with the materials for forming it in such abundance—amounting, in the ox, to 37 lbs. a day. If starch be the chief ingredient, it happens by separating a quantity of oxygen, as when starch passes into fat. Starch can become bile in no other way; but the separation being admitted, its conversion into a compound intermediate between starch and fat offers no difficulty. Thus, Professor Liebig sums up:

"The transformation of the compounds of proteine present in the body is effected by means of the oxygen conveyed by the arterial blood, and if the elements of starch, rendered soluble in the stomach, and thus carried to every part, enter into the newly formed compounds, we have the chief constituents of the animal secretions and execretions; carbonic acid, the excretion of the lungs, urea and carbonate of ammonia, excreted by the kidneys, and choleic acid, secreted by the liver."

The remarks of the author on tea and coffee will be found to possess all the novelty which characterizes the views just presented. He says:

"We shall never, certainly, be able to discover how men were led to the use of the hot infusion of the leaves of a certain shrub (tea) or of a decoction of certain roasted seeds

(coffee.) Some cause there must be, which would explain how the practice has become a necessary of life to whole nations. But it is surely still more remarkable, that the beneficial effects of both plants on the health must be ascribed to one and the same substance, the presence of which in two vegetables, belonging to different natural families, and the produce of different quarters of the globe, could hardly have presented itself to the boldest imagination. Yet recent researches have shown in such a manner as to exclude all doubt, that caffeine, the peculiar principle of coffee, and theine, that of tea, are, in all respects, identical.

"It is not less worthy of notice, that the American Indian, living entirely on flesh, discovered for himself, in tobacco smoke, a means of retarding the change of matter in the tissues of his body, and thereby of making hunger more endurable; and that he cannot withstand the action of brandy, which acting as an element of respiration, puts a stop to the change of matter by performing the function which properly belongs to the products of the metamorphosed tissues. Tea and coffee were originally met with among nations whose diet is chiefly vegetable.

"Without entering minutely into the medicinal action of caffeine (theine,) it will surely appear a most striking fact, even if we were to deny its influence on the process of secretion, that this substance, with the addition of oxygen and the elements of water, can yield taurine, the nitrogenized compound peculiar to bile."

According to this view, *caffeine* may be considered *food for the liver*, supplying that organ with the elements necessary for the performance of its function. The author assuredly threads such mazes with a firm and confident step. No difficulties intimidate him. Now that we know the active principle upon which the virtues of opium, bark, &c., depend, "it would argue," he says, "only want of sense to consider the action of these substances inexplicable," and he marches up to the solution of their modus operandi in the following style:

"With respect to the action of the other nitrogenized vegetable principles, such as quinine, or the alkaloids of opium, &c., which manifests itself not in the processes of secretion, but in the phenomena of another kind, physiologists and pathologists entertain no doubt that it is exerted chiefly on the brain and nerves. This action is commonly said to be dyna-

mic—that is, it accelerates, or retards, or alters in some way the phenomena of motion in animal life. If we reflect that this action is exerted by substances which are material, tangible and ponderable; that they disappear in the organism; that a double dose acts more powerfully than a single one; that, after a time, a fresh dose must be given, if we wish to produce the action a second time; all these considerations, viewed chemically, permit only one form of explanation; the supposition, namely that these compounds, by means of their elements, take a share in the formation of new, or the transformation of existing brain and nervous matter.

"Indeed, it cannot be considered merely accidental, that the composition of the most active remedies, namely, the vegetable alkaloids, cannot be shown to be related to that of any constituent of the body, except only the substance of the nerves and brain. All of these contain a certain quantity of nitrogen, and, in regard to their composition, they are intermediate between the compounds of proteine and the fats."

The action of certain other medicinal agents on the system is presented by the author in a novel point of view. It is known that many substances in very minute doses produce the most serious effects, and some of these, according to his views, effectuate the change by being themselves in a state of change. They originate an action which extends through the body, at the same time that their elements take no direct share in the changes which ensue. Their action on the blood is analogous to that of yeast on saccharine liquids, which is simply that of originating motion—of starting the fermentation. In this class, we find several poisons, of which we have perhaps an example in that which renders wounds in the dissecting room so dangerous. The sausage poison of Wurtemburg is considered a striking example. This poison is said to prove invariably fatal, and yet no poisonous substance can be detected in the sausages. They are in a state of fermentation, according to this theory, which the stomach fails to arrest, and which being communicated to the blood, spreads until every part capable of solution has been destroyed. By boiling, the poisonous sausage may be rendered wholesome. It is an interesting suggestion that miasms and contagions act upon the same principle—that they are organic matter, in a state of

transformation which, introduced into the living body, act as a ferment, originating changes which extend throughout the system. But we have not space to enlarge upon it.

The third part of this work treats of the recondite laws of the phenomena of motion in the animal organism. It is principally of a speculative character, but some of the speculations are so interesting that we should like to lay them before our readers; the most that we can do, however, is to present a few of the author's deductions, which we do in his own words:

"For every portion of oxygen which enters into combination in the body, a corresponding portion of heat must be generated.

"The sum of force available for mechanical purposes must be equal to the sum of the vital forces of all tissues adapted to the change of matter.

"If, in equal times, unequal quantities of oxygen are consumed, the result is obvious, in an unequal amount of heat liberated, and of mechanical force.

"When unequal amounts of mechanical force are expended, this determines the absorption of corresponding and unequal quantities of oxygen.

"For the conversion of living tissues into lifeless compounds, and for the combination of oxygen with such constituents of the body as have an affinity for it, *time is required.*

"In a given time, only a limited amount of mechanical force can be manifested, and only a limited amount of heat can be liberated.

"That which is expended, in mechanical effects, in the shape of velocity, is lost in time; that is to say, the more rapid motions are, the sooner or the more quickly is the force exhausted.

"The sum of the mechanical force produced in a given time is equal to the sum of force necessary, during the same time, to produce the voluntary and involuntary motions; that is, all the force which the heart, intestines, &c., require for their motions is lost to the voluntary motions.

"The amount of azotized food necessary to restore the equilibrium between waste and supply, is directly proportional to the amount of tissues metamorphosed.

"The amount of living matter, which in the body loses the condition of life, is, in equal temperatures, directly proportional to the mechanical effects produced in a given time.

"The amount of tissue metamorphosed in a given time, may be measured by the quantity of nitrogen in the urine.

"The sum of the mechanical effects produced in two individuals, in the same temperature, is proportional to the amount of nitrogen in their urine; whether the mechanical force has been employed in voluntary or involuntary motions, whether it has been consumed by the limbs, or by the heart and other viscera.

"That condition of the body which is called *health*, includes the conception of an equilibrium among all the causes of waste and of supply; and thus animal life is recognized as the mutual action of both; and appears as an alternating destruction and restoration of the state of equilibrium.

"In regard to its absolute amount, the waste and supply of matter is, in the different periods of life, unequal; but, in the state of health, the available vital force must always be considered as a constant quantity, corresponding to the sum of living particles.

"Growth, or the increase of mass, stands, at every age, in a fixed relation to the amount of vital force consumed as moving power.

"The vital force, which is expended for mechanical purposes, is subtracted from the sum of the force available for the purpose of increase of mass.

"The active force, which is consumed in the body in overcoming resistance (in causing increase of mass,) cannot, at the same time, be employed to produce mechanical effects.

"Hence it follows necessarily, that when, as in childhood, the supply exceeds the waste of matter, the mechanical effects produced must be less in the same proportion.

"With the increase of mechanical effects produced, the capacity of increase of mass, or of the supply of waste in living tissues, must diminish in the same proportion.

"A perfect balance between the consumption of vital force for supply of matter and that for mechanical effects, occurs, therefore, only in the adult state. It is at once recognized in the complete supply of the matter consumed. In old age more is wasted; in childhood more is supplied than wasted.

"The force available for mechanical purposes in an adult man is reckoned, in mechanics, equal to 1-5th of his own weight, which he can move during eight hours, with a velocity of five feet in two seconds.

"If the weight of a man be 150 lbs., his force is equal to a weight of 30 lbs. carried by him to a distance of 72,000 feet. For every second his momentum of force is $= 30 + 2.5 = 75$

lbs.; and for the whole day's work, his momentum of motion is $= 30 \times 72{,}000 = 2{,}160{,}000$.

"By the restoration of the original weight of his body, the man collects again a sum of force which allows him, next day, to produce, without exhaustion, the same amount of mechanical effects.

"*This supply of force is furnished in a seven hours' sleep.*"

Dr. Liebig's *theory of disease*, to which the last section of his book, but one, is devoted, is but little more than a speculation—another of the innumerable forms under which the human intellect has viewed a subject still veiled in obscurity. Some notion may be formed of his manner of treating this subject by the following extracts, which possess much interest apart from any hypothesis:

"To the observer, the action of a cause of disease exhibits itself in the disturbance of the proportion between waste and supply which is proper to each period of life. In medicine, every abnormal condition of supply or of waste, in all parts or in a single part of the body, is called disease.

"It is evident, that one and the same cause of disease will produce in the organism very different effects, according to the period of life; and that a certain amount of disturbance, which produces disease in the adult state, may be without influence in childhood or in old age. A cause of disease may, when it is added to the cause of waste in old age, produce death (annihilate all resistance on the part of the vital force;) while in the adult state it may produce only a disproportion between supply and waste; and in infancy, only an equilibrium between supply and waste (the abstract state of health).

"A cause of disease which strengthens the causes of supply, either directly or indirectly by weakening the action of the causes of waste, destroys, in the child and in the adult, the relative normal state of health; while in old age it merely brings the waste and supply into equilibrium.

"A child, lightly clothed, can bear cooling by a low external temperature without injury to health; the force available for mechanical purposes and temperature of its body increase with the change of matter which follows the cooling; while a high temperature, which impedes the change of matter, is followed by disease.

"On the other hand, we see, in hospitals and charitable institutions (in Brussels, for example) in which old people spend the last of life, when the temperature of the dormitory, in winter,

sinks 2 or 3 degrees below the usual point, that by this slight degree of cooling the death of the oldest and weakest males, as well as females, is brought about. They are found lying tranquil in bed, without the slightest symptoms of disease, or of the usual recognizable causes of death.

"Practical medicine, in many cases, makes use of cold in a highly rational manner, as a means of exalting and accelerating, in an unwonted degree, the change of matter. This occurs especially in certain morbid conditions, in the substance of the centre of the apparatus of motion; when a glowing heat and a rapid current of blood toward the brain point out an abnormal metamorphosis of the brain. When this condition continues beyond a certain time, experience teaches that all motions in the body cease. If the change of matter be chiefly confined to the brain, then the change of matter, the generation of force, diminishes in all other parts. By surrounding the head with ice the temperature is lowered, but the cause of the liberation of heat continues; the metamorphosis which decides the issue of the disease, is limited to a short period. We must not forget, that the ice melts and absorbs heat from the diseased part; that if the ice be removed before the completion of the metamorphosis, the temperature again rises; that far more heat is removed by means of ice than if we were to surround the head with a bad conductor of heat. There has obviously been liberated in an equal time, a far larger amount of heat than in the state of health; and this is only rendered possible by an increased supply of oxygen, which must have determined a more rapid change of matter."

But, despairing of doing justice by extracts to these speculations, we hasten to conclude this article by giving an abstract of his theory of respiration.

The change in the color of the blood which takes place in the lungs is one of the most familiar facts in physiology. It seems to be connected, in some way, with the iron contained in the red globules, and which is found in no other constituent of the body. The external agent concerned in this change is oxygen. The globules of arterial blood retain their florid hue until they reach the capillaries where they lose it. Venous blood in contact with oxygen is reddened, and the change, in either case, depends upon the globules which have the power of combining with gases. These globules take no part in the process of nutrition, and it cannot be doubted, therefore, that their office is to aid in respiration. They contain iron, and no

other metal can be compared with this for the remarkable properties of its compounds. Thus, the compounds of the *protoxide* of iron possess the power of depriving the other oxidized compounds of oxygen; while the compounds of *peroxide* of iron, under other circumstances, *give up* oxygen with the utmost facility. Now, in arterial blood the globules contain iron in the state of a peroxide—they are saturated with oxygen in the lungs. In the passage of the blood through the capillaries the peroxide of iron is reduced, by giving up a part of its oxygen to the carbon it there meets with, and in this way becomes a protoxide of iron, which combines with the carbonic acid just formed, and as a carbonate of iron passes on through the veins to the heart. When the globules reach the lungs, they will again take the oxygen they have lost; for every volume of oxygen absorbed they will liberate a corresponding volume of carbonic acid, and will be in a state to give off oxygen in the capillaries again. One purpose of the arterial globules is, to yield oxygen to certain constituents of the body, to produce the change of matter, determine the separation of living parts and their conversion into lifeless compounds, and aid in the formation of secretions and excretions. But the greater portion of the oxygen is employed in converting into oxidized compounds the newly-formed substances, which no longer make part of the living tissues. All the constituents present in venous blood, which have an attraction for oxygen, are converted, in the lungs, like the globules, into more highly oxidized compounds. From all of which it appears, that, in the animal organism, two processes of oxidation are going on—one in the lungs, the other in the capillaries. By means of the former, in spite of the degree of cooling, and of the increased evaporation which takes place there, the constant temperature of the lungs is kept up; while the heat of the rest of the body is supplied by the latter.

The frightful effects of sulphuretted hydrogen, and of prussic acid, when inspired, the author thinks are to be explained by the well-known action of these compounds on the preparations of iron. He says,

"Let us suppose that the globules lose their property of ab-

sorbing oxygen, and of afterwards giving up this oxygen and carrying off the resulting carbonic acid; such a hypothetical state of disease must instantly become perceptible in the temperature and other vital phenomena of the body. The change of matter will be arrested, while yet the vital motions will not be instantly stopped.

"The conductors of force, the nerves, will convey, as before, to the heart and intestines, the power necessary for their functions. This power they will receive from the muscular system, while, as no change of matter takes place in the latter, the supply must soon fail. As no change of matter occurs, no lifeless componds are separated, neither bile nor urine can be formed; and the temperature of the body must sink.

"This state of matters soon puts a stop to the process of nutrition, and, sooner or later, death must follow, but unaccompanied by febrile symptoms, which, in this case, is a very important fact.

"This example has been selected in order to show the importance and probable advantage of an examination of the blood in analogous diseased conditions. It cannot be, in the slighest degree, doubtful, that the function ascribed to the blood-globules may be considered as fully explained and cleared up, if, in such morbid conditions, we shall discover a change in their form, structure, or chemical characters, a change which must be recognizable by the use of appropriate reagents."

This view of respiration combines every quality that can be required in an hypothesis—it rests upon well-known observations, and explains all the phenomena of the process; it is probably, therefore, the true theory.

The appendix, the most elaborate part of Dr. Liebig's work, consisting of a great number of the most recent and exact analyses, and comprising the data upon which his conclusions rest, cannot, of course, be given, even in outline, in an analytical review. The work, we are sure, is destined to a wide and lasting popularity, and each reader will examine for himself the mass of curious and interesting details, upon which this beautiful system has been erected. To the chemist, the physiologist, the medical man, and the agriculturist, it is a volume of singular interest, shedding the brightest light upon every function of the animal system accessible to chemistry, and opening a path which has led to the most unexpected and astonishing results. Y.

Selections from American and Foreign Journals.

On the incipient Stage of Cancerous Affections of the Womb. By Dr. W. F. Montgomery.—In this paper the author directs the attention of practitioners to a stage of cancer uteri which precedes the two usually described by writers.

The *symptoms* are—sharp but comparatively fugitive lancinating pains in the back and loins, across the supra-pubic region, or shooting along the front of the thigh, or sometimes along the course of the sciatic nerve, producing numbness, and not unfrequently debility of the whole limb.

In a large proportion of the cases, there is found a decided fulness, or a distinct tumor in one or other iliac hollow, with fixed pain, and tenderness traceable to, and, as it were, issuing out of the abdominal ring; there is, generally, more or less irritation of the bladder, with dysuria, and the patient often complains of a sensation about the lower part of the rectum, which induces her to think that she is laboring under piles. Menstruation, though in some instances disturbed, is much more frequently quite regular in its returns; but there is apt to be bursts of hemorrhage, either accompanying the discharge, or occurring in the intervals; there is little, or no leucorrhœal or serous discharge, often none; and it is not until the disease has existed for a considerable time, that the appetite is impaired, sleep is disturbed, the flesh becomes softer and wastes, and the countenance pale, and expressive of distress.

On making the examination *per vaginam*, the margin of the os uteri is found hard, and often slightly fissured, and projects more than usual, or is natural, into the vagina, and is irregular in its form.

In the situation of the muciparous glands, there are felt several small, hard, and distinctly defined projections, almost like grains of shot, or gravel, under the mucous membrane.

Pressure on these, with the point of the finger, gives pain, and the patient often complains that it makes her stomach feel sick.

The cervix is, in most instances, slightly enlarged, and harder than it ought to be. The circumference of the os uteri, especially between the projecting glandulæ, feels turgid, and to the eye presents a deep crimson color, while the projecting points have sometimes a bluish hue.

There is no thickening, or other alteration of structure in any part of the vagina, at its conjunction with which the cervix uteri moves freely; nor is there any consolidation of the uterus with the neighboring contents of the pelvis; in fact, the morbid organic change appears to be, at first, entirely confined to the os uteri and lower portion of the cervix.

This stage of the affection is, in many instances, *very slow*, lasting sometimes, *for years*, before the second and hopeless stage is established; during this time the patient experiences only comparatively slight and transient attacks of pain, or perhaps only sensations of uneasiness, referred often to the situation of one or other of the ovaries, or about the os uteri, with anomalous tingling along the front and inside of the thighs; these last for a few hours, or a day or two, and then disappear, perhaps for weeks; but again and again return in the same situation, and for a long time are not increased in severity; the patient finds that sexual intercourse now, occasionally, causes her pain, which she ascribes to some deep-seated part being touched, and the act is followed by an appearance of blood; she is, also, often troubled with slight irritability of the bladder; but the appetite, digestion, and sleep, may, for a long time, continue good, and the pulse, generally, gives no indication of the existing disease, or its changes; an observation which will be found applicable to many uterine affections of a grave character; in short, the general health may long remain quite undisturbed, nor has the patient, in many instances, the slightest suspicion that there is any thing seriously wrong with her, nor thinks of seeking for medical aid until she is induced to do so by the solicitations of her husband, or some anxious friend who has become, as she thinks, unreasonably alarmed about her state.

Dr. Montgomery thinks that the first discoverable change in the cases now alluded to "takes place in and around" the muciparous glandulæ, which exist in such numbers in the "cervix and margin of the os uteri;" these become indurated by the deposition of scirrhous matter around them, and by the thickening of their coats, in consequence of which they feel, *at*

first, almost like grains of shot or gravel under the mucous membrane.

Treatment.—In almost every instance, the treatment should be begun by the *local abstraction of blood*, either by cupping, or by leeches applied to the os uteri, or as near as possible to the organ; and their application will, in most cases, require to be frequently repeated, and should be accompanied by the free use of anodyne fomentations. *Venesection* is not, in general, required. Except there be something specially to forbid its use, *mercury* should be given, in some form, so as to bring the system very gently, but decidedly, under its influence; for which purpose it may be combined with iodine in very minute proportions, with camphor, opium, hyoscyamus, or hemlock; and occasionally by friction, especially where there exists evidence of inflammatory action in the iliac hollow, as already adverted to.

Afterwards, *iodine* or *hydriodate of potash* may be used both internally and externally; and *iron* will be found a most beneficial and powerful agent, especially in the form of the saccharine carbonate, or the carbonate given in the nascent state. The *iodide of iron*, which combines, to a certain degree, the powers of both remedies, may also be used with some advantage in most cases. *Counter irritation* is an agent of very great influence in this complaint, and may be established in a variety of ways, which it is unnecessary to enumerate; but a very effectual mode is by making a small blister over different parts in succession, and keeping it discharging freely for several days, by the application of the French dressing, or Albespeyer's papers.

After the removal of the congestion and organic changes from the os uteri, there remains, occasionally, a sensitiveness of the part, which causes the patient much discomfort, and which will be best relieved by the use of the bath, as above directed, conjoined with anodyne applications to the part, or the nitrate of silver in solution; the best mode of applying which, is by means of a bent glass tube of about an inch in diameter, which the patient can introduce and manage for herself; all that is necessary is, that she would lie on her back, and introduce the tube as far as its curvature, and then pour into the upper end the medicated solution, which will immediately pass to the os uteri, and can be retained there as long as necessary, the tube filling the vagina sufficiently to prevent its flowing away, which is a great advantage.

The patient should be strictly enjoined to avoid every thing that could stimulate the uterus—such as riding on horseback,

&c.; but, especially, she should refrain from indulgence in sexual intercourse. Wine, if used at all, should be of a very mild kind, and very sparingly taken, and the same rule should apply to malt drinks; the stronger kinds of ale and porter should be altogether prohibited.

No circumstance connected with the treatment of this affection requires more scrupulous attention than the regulation of the patient's habits and modes of living; indeed, if this be not very carefully managed, all other measures will most probably be defeated.

In illustration of the foregoing remarks, Dr. Montgomery relates several cases which terminated successfully under the treatment laid down by him. Further researches, however, are required to establish that the nature of the disease is truly cancerous.—*Prov. Med. and Surg. Jour.*, from *Dub. Jour. Med. Sci.*

Styptic Effect of Kreosote.—A robust countryman divided the ulnar artery with a sharp knife, the consequence of which was repeated bleedings, which, however, were staunched by surgical aid. Three weeks afterwards, hemorrhage returned, and Dr. Burdach of Luckan was sent for. He found the wound, which at first was a simple puncture, livid at the edges, and expanded to the size of the palm of the hand, by a spongy growth from the bottom. This spongy mass was in a gangrenous condition, and prevented the examination of the wound, the arm was swollen from the shoulder to the finger points; it could not be moved, and was excessively painful. Dr. Burdach had only the choice between actual cautery and kreosote left, for in such a state of the arm, tying the artery was out of the question. He poured 3ss of kreosote (*freed from eupion*) into the wound. This gave the patient no pain; nay, after it he enjoyed refreshing sleep for the first time after the accident. There was no more hemorrhage; the pouring in of the kreosote was repeated morning and evening, and the spongy mass gradually diminished, and three days afterwards, under the cooperating influence of bandages moistened with kreosote, ol. tereb., and balsam. indic., loosened itself from the bottom of the wound. The divided artery was no more visible, the swelling of the arm decreased, and the complete cure shortly followed without any relapse.—*Lond. and Edin. Monthly Jour. Med. Sci.*, May 1842, from *Medicinische Zeitung*, Jahrg. 1840. No. 31.

The late Baron Larrey.—The death of Baron Larrey, which took place at Lyons on the 29th of July, has deprived French Surgery of one of its brightest ornaments. Few members of our profession have played so important and distinguished a part in the annals of both history and science, as the veteran Larrey. His career was connected with some of the most stirring events of the past half century, and his reputation belongs hardly more to his profession than to humanity.

Jean-Dominique Larrey was born in 1766, in the village of Baudeau, in France. Left early an orphan, at the age of fifteen he was placed under the care of a paternal uncle, who practised surgery at Toulouse, with whom he spent seven years in the study of medicine. In 1787, he obtained the post of Surgeon in the Navy, and made a cruise to the colonies. Returning shortly after to France, in the midst of the excitement of the revolution, he became attached, as an *Interne*, to the hospital of the Invalids. After remaining some years, he obtained the rank of full-surgeon, and joined the army of the Rhine. Larrey soon signalized himself in the army by the introduction of the *ambulances volantes*, the improved transports for hospital stores, which have been found so useful in the French service. Larrey's services and his *ambulances volantes* were considered of so much importance, that, after the battle of Mayence, in 1793, they were publicly noticed in the bulletin issued by General Beauharnais.

In 1794 Larrey was appointed Surgeon-General, and repaired to Toulon, where commenced his intimacy with Napoleon, then a lieutenant of artillery. The school of military surgery at Val-de-Grâce had been previously established, and Larrey appointed a Professor; but professors and pupils were soon called to active service, and the school was suspended. In 1798, Larrey joined the army of Egypt. His account of the memorable campaign in Egypt and in Syria is familiar to every professional reader. Larrey accompanied Napoleon in nearly all his campaigns, and was honored by him with repeated marks of distinction. In 1812, he received the title of Surgeon-General of the *Grande Armée*. His fidelity to the emperor brought him into active service during the hundred days, and he was present at Waterloo, where he was wounded and made prisoner. During the restoration, M. Larrey's well known attachment to Napoleon, kept him under a cloud, but the revolution of 1830 restored him to the public service, to which he devoted himself to the close of his life. His death

at Lyons took place upon his journey home from the army in Algeria.

Baron Larrey's scientific was not less distinguished than his military career. His writings on military surgery have been long recognized as the highest authority on the subject, and certainly no surgeon of his day equalled him in the amount and variety of his contributions to practical surgery. In 1803 he published his "*Historical and Surgical Account of the Campaign of the Army of the East, in Egypt and Syria.*" In 1812, he published three volumes of his admirable "*Memoirs of Military Surgery and Campaigns,*" adding a fourth volume in in 1817. Besides these, Larrey published numerous other works, including four volumes of "*Surgical Clinique derived chiefly from Camps and Hospitals, from* 1792 *to* 1832."

The merit of the adaptation of the *immoveable apparatus* to the treatment of fractures and certain wounds of the soft parts, belongs, we believe, to M. Larrey. The advantages of this mode of treatment in the surgery of actual warfare can hardly be disputed, whatever may be our views of the practice under ordinary circumstances, In his last work, published shortly before his death, M. Larrey gives a new notice of the apparatus, and sets in a striking point of view the advantages of its adoption in military surgery.

Baron Larrey's writings possess the double attraction of historical and scientific value. An actor in scenes, the grandeur of which has never been surpassed, his narrations constitute not only invaluable records of practical surgery, but they, at the same time, form an interesting chronicle of a most eventful epoch of European history. On the great merit of Larrey's writings on military surgery, it is unnecessary to dwell. His immense clinical experience, derived from a theatre that has never been equalled in magnitude, gives them an importance and authority that it would be presumption to question. He was perhaps, in some respects, too pertinacious an adherent to the opinions of by-gone times, and did injustice to the importance of recent discoveries; but his works are all entitled to the praise of practical value, and are distinguished by a lucid detail of facts, a just discrimination of their relative importance, and the evidences of a sound judgment.

Larrey's career was as virtuous as it was memorable. He enjoyed, pre-eminently the confidence and respect of Napoleon, who spoke of him at St. Helena in the warmest terms. His allusion to him in his will is well known.

Baron Larrey was interred at Père-la-Chaise, with great so-

lemnity. A number of distinguished men followed him to the tomb, and orations were delivered by MM. Pariset, Breschet, and others. A subscription has been opened to erect a monument to his memory.—*Med. Examiner.*

M. Louis on Typhoid Fever.—We observe by the French journals that a new edition of M. Louis' well known work on this subject has recently been published in Paris. Its title deserves notice: 'Recherches Anatomiques, Pathologiques et Therapeutiques sur la Maladie connue sous les Noms de Fievre Tyhoide, Putride, Adynamique, Ataxique, Bilieuse, Muqueuse, Gastro-enterite, Enterite Folliculeuse, Dothinenterite, &c., comparée avec les Maladies Aigues les plus ordinaries!' Does not this alone indicate how bewildered the ideas of medical men must still be on the all important subject of fever? We had almost hoped that M. Louis would by this time have found reason to modify his opinion as to the seat or proximate cause of typhoid fever being in the intestinal canal; but we find that we are mistaken. He still maintains, and even in more decided terms than ever, that 'the anatomical character of the typhoid affection (a very stupid phrase) consists in a morbid alteration of the intestinal, chiefly the Peyerian glands; and, also, that any secondary lesions are very usually of an inflammatory nature.'

This doctrine is a most fallacious, and unfortunately at the same time a most hurtful, one. While we admit that the intestinal lesions seem, from all accounts, to be of much more frequent occurrence in fever as it occurs in France, than as it is seen in this country, we most confidently deny that they are uniform or constant even in the former case—at least if we can trust the testimony of such men as Chomel and Andral. But even admitting the frequency of intestinal lesions in fever, does it necessarily follow that they are the consequences of simple inflammatory action, as asserted by Broussais, Louis, Bouillaud, and others of this school? Do we observe the same phenomena on dissection in typhus as are found in fatal cases of genuine enteritis? Assuredly not. The patchy redness in different parts of the canal, the tumefied and softened condition of the mucous and submucous tissues, the enlargement, ulceration, and even sphacelation of the follicular glands, &c., cannot surely be admitted as the legitimate evidences of a pure phlegmasia. They might rather be regarded as the results of the irritation of unhealthy acrid secretion on the mucous

surface than as the primary and essential phenomena of the disease. But without going so far as this, we have no hesitation in asserting that the intestinal lesions, however frequent they may be in the fevers of Paris, can only be viewed as one of the changes induced by the general disease of the system. To apply the term of *enteritis* to such lesions is not only incorrect in theory, but most pernicious in practice. It has been from the too general adoption of this dogma by the present generation of medical men in France, that so many and so serious errors have been committed by them in the treatment of their patients. When we read the grave discussions that are frequently agitated in the Academy of Medicine of Paris, on the results of some novel mode of treating typhoid fever, what are we to expect from the young surgeons and physicians who are annually drafted away either to the country, or perhaps into the army and navy, immediately after having had their heads crammed with the 'nouvelle doctrine' as taught by the professors of the physiological school!

Part of M. Louis' work is taken up with showing that 'jail-typhus and typhoid fever are identically the same disease.' What rational man ever doubted it? But such are the vague opinions of French medical men on the subject of fever, that M. Louis deems it necessary to devote an entire chapter to prove a position, which has been recognized as an acknowledged truth by British practitioners ever since, and indeed long before the days of Huxham. But then it is at the same time well known that almost every epidemic of fever, whether it appears in jails or other crowded buildings, or in the lanes and dirty alleys of a city, exhibits some peculiarity or other by which it may be characterised; and yet no sensible physician would dream of designating one epidemic as cephalitis, another as bronchitis, a third as gastro-enteritis, merely because the predominant symptoms may be at different times seated in the head, chest, or abdomen. Until medical men can be induced to carry their thoughts beyond the more obvious lesions discoverable on the dissection of patients who fall a victim to typhus fever, they will ever remain involved in a labyrinth of perplexities, which will only be increased by a more extensive acquaintance with the disease. A calm unprejudiced examination of the history of typhus must lead every one, we are inclined to believe, to the conviction that it is the result of an invisible atmospheric agent—call it miasm, malaria, matter of contagion, or what you will—on the human body, acting primarily on the nervous system, and often ra-

pidly absorbed into the system, thereby inducing a more or less decidedly vitiated state of the circulating fluids.

The lesions of the viscera, whether of the cranial, or of the thoracic or abdominal cavities, are all secondary or consecutive phenomena of the disease; their occurrence being attributable either to the contemporaneous influence of other atmospheric agencies, to the idiosyncrasy, habits, and previous state of health of the patients affected, or perhaps to the differences in the nature and degree of the aerial poison that has been introduced into the system. We must admit that, in taking this view of the question there are many points that cannot be demonstrated by actual proofs, and that the theory is essentially based upon a mere probability; but let us remember that there is, and ever will be, much in the history of these diseases, especially those of an epidemic and diffusive character, that must defy the scrutiny of human observation: we cannot *exhibit* the infectious poison of scarlet fever, or of measles, or the miasma which gives rise to intermittent or remittent fevers; and yet no one disputes their existence. It is therefore a very insufficient reply to be told that we cannot demonstrate our theory of the disease. Some modern French writers are constantly trying to make us believe that medicine is capable of being rendered an *exact* science, and that we must not admit any positions in our reasonings on disease but what can be proved by actual observation. This is a most foolish and dangerous doctrine. As in moral and political philosophy, so in medicine, there are no permanently fixed and unalterable rules which are inevitably true at all times and under all circumstances. The beauty of a moral precept or the expediency of a political maxim may charm the mind in theoretical speculation; but in our practical dealings with the world, we find, alas! that the line of conduct to be adopted must greatly depend upon the circumstances of each case —ever striving however to act on the principle of doing unto others as we wish them to do unto ourselves.

Dr. Landouzy, of Rheims, the reviewer of M. Louis' work in the French Medical Gazette, states that his own experience in the late epidemic of jail-typhus in that town confirms the opinion that it is analogous, if not identical, in nature with the typhoid fever of the metropolis, and he bases his opinion on the circumstance of the *constant* existence, in all cases and at all periods of the disease, of the intestinal lesions in both forms of the fever. Yet, strange to say, he informs us that in no case were there any symptoms of such lesions during the

life: 'la diarrhée, la douleur abdominale, le meteorisme et le gargouillement ne se sont jamais rencontrés.' How can Dr. Landouzy, or any other sensible man, in the face of such a statement as this, continue in the belief that the disease, in which these symptoms were absent, was an inflammatory affection of the mucous coat of the bowels; in other words, that it was a 'gastro-enterite,' or 'dothinenterite.' He suggests, as a novel idea, that the difference in the symptoms of typhus fever in different times and seasons may be connected with some difference in the type or genus, as he calls it, of the disease, and very coolly remarks that 'the observations of authors on this point of pathology having in truth but little or no value until the 'beaux travaux' of M. Louis have become generally known, we must wait for new epidemics of typhus before we can offer a positive opinion upon the subject.'

Has Dr. Landouzy never heard of Sydenham or Huxham, or of Cullen, or of his own countryman Pinel, not to mention a century of other authors, who have all insisted upon this essential fact connected with the history of fevers—the differences in the character of the disease according to what has been demonstrated the 'medical constitution' of the season? Little is the wonder that the French are making discoveries in medicine almost every day: they seem to be utterly unaware of their own well expressed motto, 'nothing is so new as that which is forgot!'

The therapeutic portion of this edition of M. Louis' work has received, we are informed, 'd'immenses developpemens;' but the reviewer does not tell us particulars; all that he says is, that 'the author *seems* to prefer in the greater number of cases the use of evacuants to any other remedies.' The little word *seems* (semble) implies, if we are not much mistaken, a mighty deal in reference to M. Louis' mode of treatment. It is well known that this gentleman, however high he may be regarded as a morbid anatomist, cannot be appealed to as an authority on practical subjects. Some years ago, as we have noticed in a former article, he made the important discovery that bloodletting and blistering had little or no efficacy in subduing pneumonia! He has probably extended the application of this discovery to the treatment of fevers, and may have found out that by far the best mode of managing them is to do nothing.

In this respect his example offers a marked contrast with that of M. Bouillaud, a disciple of the same pathological school, whose 'grande decouverte' is the 'nouvelle formule' of bleeding 'coup sur coup,' so as to 'juguler' the disease at

once!! But we trust that it is quite unnecessary to expose the fallacy of this gentleman's views at present, as we have so repeatedly of late years cautioned the English reader against the errors into which he is apt to lead the inexperienced by the bold and confident manner with which he proclaims his marvellous success.—*Med. Chir. Review.*

Ligature of the primitive Iliac Artery.—The primitive iliac has been lately tied with entire success, at the Pennsylvania Hospital, by Dr. Edward Peace. We are enabled to present our readers with the following particulars of the case.

Israel Jones, a labourer, was admitted into the surgical wards of the Pennsylvania Hospital on the 22d of August, 1842, for an inguinal aneurism of five months standing. Five months previous to his entrance he strained his right groin, while lifting a heavy stone. A few days subsequently a hard tumour, about the size of a pea, made its appearance, which became as large as a walnut in the course of a month, and continued to increase until the end of the fourth month, when it attained its maximum growth. There was pulsation about the third or fourth week. About the beginning of the fourth month numbness and pain commenced in the tumour, and extended along the anterior portion of the thigh. The latter symptoms were always aggravated by exercise, and abated by rest. The man continued his daily occupations until three weeks previous to his entering the Hospital, when his sufferings became so great as to oblige him to desist. The pain at this period was so acute as to deprive him of sleep, and obliged him to maintain night and day a sitting posture, with his leg flexed on the thigh, and this on the pelvis; the whole limb resting on its exterior aspect. The tumour was large and irregular, hemispheroidal, and was, at least, two inches in height, its vertical diameter five and a half inches, and the transverse diameter about the same. It appeared to involve nearly all the right external iliac, together with some two inches of the femoral artery of the right side.

The man was an excellent subject, in the prime of life, robust, temperate, and uniformly healthy.

The operation was performed on the morning of the 29th of August, by Dr. E. Peace, assisted by his colleagues, Drs. Randolph and Norris, and Dr. J. Rhea Barton, and occupied forty-seven minutes. It was performed in the following manner:—A semi-elliptical incision was made, commencing

over the anterior spinous process of the ileum nearly on a level with the umbilicus, and directed obliquely downwards to within half an inch of the external abdominal ring, and nearly parallel to Poupart's ligament. The incision was seven inches in length. The integument, the fascia of the external oblique, the external oblique, and the fascia of the internal oblique, were divided with the bistoury. The transversalis and internal oblique muscles were now exposed, and with the aponeurosis were divided on a director. The peritoneum was then separated with some difficulty, and the vessel brought into view.

The vessel was taken up about half an inch above the bifurcation. the ligature being passed around it very readily by means of Gibson's needle. Pulsation and pain ceased in the tumour the moment it was tied.

Numbness of the limb and foot, and insensibility, particularly of the toes, supervened immediately. Numbness continued to some degree, with occasional intervals, throughout the first two weeks. Sensibility gradually increased until the third day, when it was entirely restored, even in the toes. The limb below the knee became sensibly cold within an hour after the operation. It was immediately enveloped in carded wool, and recovered its natural temperature, as far down as the ankle, within the first twelve hours. At the end of twenty-four hours warmth had returned in the foot—the toes only remaining below the proper standard of heat. As the heat returned in the limb it augmented, so as to make it really warmer than the sound one, which continued to be the case during the first two weeks.

The capillary circulation in the the toes continued sluggish until the sixth day, when it appeared to be entirely restored to activity. The man experienced slight pain, with the numbness, from time to time in the affected limb, but did not suffer materially until about the middle of the second week. At this time he complained of severe pain, beginning at the toes and darting up into the tumour. This pain was relieved by the application to the tumour of lint wet with laudanum. The tumour, which had previously been rather soft, at this time became much more dense, and decidedly smaller.

No other symptom worthy of especial note presented itself, except some tumefaction of the leg, which occurred on the fifteenth day and subsided in two days.

The wound was dressed on the fourth day, and every day thereafter. The discharge was always healthy and very moderate. The man's appetite excellent, and general health im-

proved. One-half of the wound united by the first intention; and the whole wound, except the sinus occupied by the ligature, had cicatrised within the first two weeks.

The ligature came away on September 27th, the *thirty-fifth day.* The patient is now allowed to sit up, and is doing extremely well.

The man experienced great relief from the numbness and pain, in repeated friction of the whole limb, (and especially the foot, which suffered the most in this way,) with soap liniment.

This, we believe, is the ninth time the primitive iliac has been tied. It was first tied in 1812, by Professor Gibson, of the University of Pennsylvania, for a gun-shot wound. The patient died from peritoneal inflammation on the thirteenth day. 2. By Professor Mott, in 1827. The ligature came away on the eighteenth day, and the patient recovered. 3. By Sir Phillip Crampton, in 1828. Death on the fourth day from hemorrhage. 4. By Mr. Liston, in 1829, for secondary hemorrhage after amputation. The patient, who was 8 years old, died. 5. It was tied in 1833, by Mr. Guthrie, for supposed gluteal aneurism. The operation was successful. The patient died eight months subsequently, and the disease proved to be a medullary tumour. 6. In 1837, Mr. Salomon, of St. Petersburg, tied the primitive iliac with success. 7. Mr. Syme, of Edinburgh, performed this operation in 1838. The patient died on the fourth day. 8. By M. Deguise, at the hospital of Charenton, Paris, in 1840. Successful.

Note.—Dr. Reeve, the Editor of the American Edition of Cooper's Surgical Dictionary, states that the primitive iliac was tied about six or seven years ago by Professor Bushe, in a child two months old, for congenital aneurism of one of the labia. Death occurred from abscess of the knee-joint in a few weeks.—*Philadelphia Medical Examiner.*

Devilliers and Chailly on Auscultation in Pregnancy.—To determine the position of the maximum intensity of the double sounds of the fœtal heart, we divide the abdomen into four parts by two lines crossing one another perpendicularly at the umbilicus, one falling from the extremity of the xiphoid cartilage to the symphysis pubis, the other running horizontally from side to side. In cranial presentations, the sounds will be heard most intensely below, in presentation of the pelvic extremity above the transverse line; the precordial region being nearer to the head than the pelvis. It is very diffi-

cult to diagnose presentations of the trunk, inasmuch as the sounds being heard below the transverse line, they may be confounded with cranial positions. The maximum intensity of the sounds on the right or left of the perpendicular line indicates a right or left position of the head. The more distinctly the sounds are heard in the median line, the greater is the probability that the abdomen of the child is directed anteriorly. In the opposite position the sounds are heard more clearly in a lateral situation. But these signs, especially in the latter case, are very difficult, and we are often obliged to have recourse to other means to assist in the diagnosis. A table is added, according to which a correct diagnosis was formed by auscultation in 223, a false one in 68 cases of pregnancy. The large proportion of the latter was owing to the difficulty in determining the anterior or posterior, and right or left positions of the child.—*London Med. Gaz.*, from *Revue Médicale.*

Post-Mortem Examination of a Soldier one hundred and two years old.—This man had taken part in numerous campaigns, and had never suffered from illness. He retained his bodily powers up to his hundredth year, but gave up from decrepitude and mental incapacity fifteen months previously to his death, and was received into the Military Hospital of Bradenburgh. His death occurred unexpectedly and without premonitory indication.

On examination, thirty-six hours after decease, the body was found moderately stout; the few hairs that remained were silvery; his teeth had long since quitted their sockets, the alveolar processes of the jaws were completely absorbed, and the mouth contracted in size. The opening of the cranium was rendered difficult by the thickness of the bones and their adhesion with the dura mater, and the latter was dense and tendinous. The brain was normal, its arteries converted into bony tubes; the pineal gland was filled with calcareous matter, and the choroid plexuses were vesicular. The larynx and carotida were completely ossified. The thorax large and well formed; lungs perfectly sound; heart normal; valves remarkably ossified; large arteries converted into ossific tubes; costal cartilages almost completely ossified. The liver, spleen, and kidneys presented a state of extreme softening, greater than could be referred to decomposition, and which can alone be explained on the supposition of excessive debility of the vital powers.

Thus the kidneys consisted of a semifluid granular mass, although the urine was natural and unclouded. The gall-bladder was distended with upwards of five hundred concretions, of various magnitude, and its coats were so soft that they became ruptured by the slightest touch. No symptoms of this collection had existed during life.—*London Lancet*, Aug. 13, 1842, from *Medicinische Zeitung*, 1842.

Lime Moxa.—Lime has been employed by Dr. Osborne as a moxa.

It is used as follows: A little quicklime, to the depth of half an inch, is put within a *porte moxa*, or a circular piece of card, and applied to the skin; water is then dropped on it, and mixed. In about two minutes it swells, and an intense heat is given out. It has been calculated that the heat produced is equal to 500° of Fahrenheit. If it is kept on as long as the heat continues, the whole skin is destroyed; but by removing it sooner, an issue to any extent may be formed. Dr. O. prefers this kind of moxa, from the great heat suddenly produced, and from no fire or sparks being seen, so as to alarm the patient. To ascertain the depth to which this moxa acts, he applies it to the surface of an egg, and then observes the thickness of coagulated albumen formed beneath. The size of the ulcer formed is always twice as large as that of the lime applied. When the lime is prepared from calcareous spar, the heat produced on the addition of the water is sudden and intense, and the pain is proportionately urgent. For ordinary purposes, however, well-selected pieces of lime, from a lime-kiln, answer well if *fresh*, but not otherwise.—*Lond. and Edin. Monthly Journ. Med. Sci.*, March, 1842.

Sciatica cured by Extract of Belladonna.—The following interesting case is related in the *Bulletin Thérapeutique.*

A lieutenant in the French navy had long laboured under a very severe form of sciatica; the pain extended from the sciatic notch to the terminal branches of the nerve in the foot, and was of the most violent kind. Several remedies had been tried without effect, when M. Hiriart resolved on employing the extract of belladonna. The bowels were first cleared out by an active purgative; and the whole limb was then rubbed, several times during the day, with an ointment composed of

one part of the extract to two of lard. After the fourth friction the patient experienced a creeping sensation in the limb, and some slight symptoms of narcotism appeared; he enjoyed, however, some sleep during the night. On the following morning the pain had shifted to the opposite limb, whence it was driven by the same means. The state of the bowels and stomach was regulated by gentle purgatives and proper diet, and in a short time the patient was completely cured of a disease from which he had previously suffered the most cruel torments.—*Prov. Med. and Surg. Journ.*, March 26, 1842.

Removal of Discoloration of the Skin from Nitrate of Silver.—Dr. Patterson considers that "there can scarcely be a doubt, that in those cases, where the skin has become discoloured from the long use of nitrate of silver, the discolouration may be removed by the internal and external employment of suitable preparations of iodine."

The following is the formula which Dr. Patterson employs for the administration of the ioduret of silver.

℞. Iodureti Argenti,
Nitratis Potassæ, *aa* gr. x.,
Tere simul ut fiat pulvis subtil., dein adde
Pulv. Glycyrrhizæ ʒss.
Sacchari Albi ℈j.
Mucil. Arab. q. s. M.
Fiant pil. xl. quarum sumat æger j. ter in die.

Employment of the Ergot of Rye in Cases of Paraplegia. By M. Payan, of Aix.—M. Payan begins by stating that he has recenntly sought to demonstrate that it was wrong to consider ergot of rye as a simple excitor of uterine contractility; that facts prove that ergot acts on the rectum, the bladder, and the inferior extremities, when these parts are in certain asthenic conditions, in the same manner as on the womb in cases of uterine inertia; and that, not being able to attribute rationally to this agent specific effects on parts of the body essentially different, he was obliged to seek for its action still further, and to refer it to some organ holding still under its influence these different parts. M. Payan has discovered that it is on the spinal marrow that the ergot exercises a special and primitive action.

This fact being established, it became evident that we ought to recur to the use of ergot in paraplegias and debility of the inferior extremities, proceeding from causes which have suspended or weakened the action of the spinal marrow, without having altered its texture.

M. Payan adds the three following facts to those which he has already published, and which demonstrate the justice of his views:

1. A man of 40 years, fell on the perineum, and paraplegia was the consequence. The patient, treated at Marseilles, was completely cured. Some time after, in a journey which he made to Aix, he again experienced the same symptoms, and entered the Hotel Dieu of that city. In the absence of M. Payan opiate liniments, blisters, &c., were unsuccessfully used against the paraplegia. The case then came under M. Payan's care, who substituted for these means one gramme* of the ergot to be taken once. Twelve hours after the paralyzed limb became agitated frequently by muscular starts, and he daily recovered strength. At the end of six days the patient could walk with the aid of a single crutch. During a fortnight the ergot was administered in the dose of two grammes. A month after, the treatment having been suspended on the fifteenth day, on account of the appearance of some slight gastric derangement, the amelioration persisted, but the patient left the hospital.

2. A man of 30, attacked with a complete paraplegia, committed himself to the care of M. Payan. A number of energetic measures had already been tried. This was his state when he was submitted to the observation of this skilful physician.

The two lower limbs support well enough the weight of the body; but he finds it impossible to walk any time without sitting down; unless he does so he infallibly falls. The right lower extremity is strong enough, but insensible; the other limb is sensible, but a little atrophied. The bladder has lost a part of its contractility.

A gramme of the ergot was administered every morning. At the end of a few days the dose was increased to two grammes. At the same time frictions with a stimulating liniment were made along the spine. After eighteen days the two lower extremities had become strong, and the patient could return into his country.

3. A workman, having endured a severe paraplegia, the

* Equivalent to 15 grs. English.

consequence of a bad attack from lead, different means had failed to relieve the malady; the ergot completely triumphed over it.

From these three cases M. Payan not only infers the efficacy, but, moreover, the complete innocence of this medicine, which has been ordered for six weeks without any accident, which this year has been given for forty consecutive days, in doses of forty to eighty centi-grammes, to a young girl five years old, without her being the least injured by it.—*Dub. Jour. Med. Sci.*, from *Journ. de Pharm.*

The employment of Strychnia in Amaurosis.—A laboring boy, 12 years old, received a blow in the right supra-orbital region, by the falling of a pewter vessel which he was endeavoring to remove from a high shelf. At the moment of receiving the blow he perceived a flash of light in the eye, but could see nothing with it afterwards. In three hours he came under the care of Dr. Dusterberg, of Lippstadt. Immediately above the right eye-lid was visible a small blue spot, of the size of a horse-bean. In the eyeball itself nothing abnormal could be detected, no trace of opacity or extravasation of blood. The pupil acted naturally, as in the sound organ, but the power of vision was entirely lost in the right eye; so that he was unconscious when it was directed toward the full glare of the sun. He was treated for two months with bleeding, cold applications, mercurial frictions, blisters, drastics, emetics, electricity, and even the frontal nerve was divided; but all in vain; the amaurosis did not in any degree yield. Subsequently a solution of a grain of nitrate of strychnia in half an ounce of rectified spirit of wine was dropped into the eye four or five times daily: the result of which was, that, in fourteen days, sensations of light were experienced in the affected eye, which, under the continued use of the remedy, increased so that he was enabled to distinguish colored objects. After a period of three months, the power of vision had so far returned that he could recognize bodies at a distance of three feet. At this point the improvement stopped, notwithstanding that the dose of strychnia was increased, and its endemic application had recourse to. The case, however, may be fairly adduced to show the beneficial influence of strychnia on torpid amaurosis.—*Lond. Med. Gaz.*, Aug. 5, 1842, from *Schmidt's Jahrbücher.*

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LOUISVILLE, NOVEMBER 1, 1842.

KANE'S ELEMENTS OF CHEMISTRY.

Professor Draper has rendered an acceptable service to American students of Chemistry, by giving them this edition of Dr. Kane's admirable work, with alterations, adapting it better to the system of instruction pursued in this country. We have no hesitation in agreeing with the editor, that, as a text-book, it is the best work extant in the English language. It represents the present condition of chemical science, and exhibits its applications to pharmacy, medicine, and the useful arts. In a work of so great detail much is given in which the student can feel but little interest. Dr. Draper has had the details printed in a smaller type, by which the learner will be able to distinguish the parts embracing the leading principles upon which he will bestow his first attention. The mechanical execution of the work is excellent. It contains a great number of wood cuts, which will materially aid the student in comprehending processes. So rapid is the march of chemical science, especially of organic chemistry, that it must be posted up at short intervals. New publications are demanded every year to bring up the improvements. Kane's Elements embrace an account of many which are not to be met with in previous

works on chemistry, and it will be found that he has adapted his work admirably to the wants of medical men, by pointing out the connexion, where any existed, between his subject and pharmacy, physiology, or pathology. Y.

PHRENOLOGY AND NAPOLEON.

Napoleon's bust, as it is to be seen in phrenological cabinets, must have struck every one as very unlike the head of a great warrior. In fact, it is a very *mediocre* thing, and we have always felt, that, if it represented faithfully the size and form of the emperor's head, it was exceedingly out of place in a phrenological cabinet, since it set fairly at defiance every principle of organology. We have heard various attempts at explaining the discrepancy. It has been said, that, after Napoleon's exile to St. Helena, his brain shrunk from inaction, and that, consequently, the bust taken after his death does not give the proportions of his head as it was in the prime of his life. Another explanation is, that Antomarchi was an *anti-phrenologist*, and published a false bust of his illustrious patient. The true account of the matter is probably given in the following anecdote, which we lately met with in a biographical sketch of Dr. Graves, of Dublin, and for which we are sure he will receive the thanks of all phrenologists.

"When Napoleon died, there was no plaster of Paris in St. Helena, and no one who knew how to take a plaster bust of the emperor, which he had never allowed any one to do during his life (why, it is difficult to say). The emperor's suite expressed great regret at this circumstance, in the presence of Doctor Burton of the 66th regiment, a relative of Dr. Graves (who had all the particulars from him). Doctor Burton had assisted in the post-mortem examination of Napoleon—his name is to the attested certificate of that investigation—and when he heard these expressions of regret, he offered to make all possible efforts to form a bust, on the condition, that if he succeeded he should have the original, and the emperor's attendants a copy. He had in so hot a climate, to hasten his operations, and was out all night in a boat seeking sulphate of lime (gypsum), which he had previously observed in veins on the rocks near the shore. He collected a sufficient quantity, prepared it, and after much labor succeeded in taking an excellent cast of Napoleon's bust. Next evening he went to town, having labored all day, to take some refreshment in his quarters, leaving the mould at Longwood. Some of the officers, hearing from him what he had done, said he would never see the mould again. To this he

answered, that he did not believe any effort would be made to rob him of his rights, which he had taken care to verify by a written contract; but when he returned to Longwood, he found the mask or face part of the mould had been stolen. The rest was left—its use either being not understood, or being too heavy to get away privately. No entreaties of Dr. Burton were availing to recover the mask. Madame Bertrand said she would never permit an "Englishman to *publish* the emperor." This mask was published by Antomarchi, Napoleon's physician, and not only the mask but the whole bust. Except the mask, every other part was fictitious, as Doctor Burton secured the rest, and yet throughout Europe, the imaginary bumps, in this supposed cast of Napoleon's cranium, have proved quite satisfactory to phrenologists as affording undoubted proofs of the accuracy and truth of their science. Doctor Burton died suddenly on parade, at Canterbury, while doing duty with a regiment of hussars to which he had been appointed surgeon; and the mould, there is too much reason to believe, was destroyed in his quarters; his servants being ignorant of its value."

Y.

THE

WESTERN JOURNAL

OF

MEDICINE AND SURGERY.

DECEMBER, 1842.

ART. I.—*The Northern Lakes, a Summer Resort for Invalids of the South.* By DANIEL DRAKE, M. D., Professor in the Medical Institute of Louisville.

Much having been written on the comparative fitness of different places in the South, as winter residences for the invalids of the North, we propose to say something for the benefit of the valetudinarians of the former. Their present places of resort are chiefly the following—1. The Virginia Springs, where they *can* drink mineral waters of different kinds, and *must* breathe a mountain air which, although not in a high latitude, is invigorating. 2. The New York Springs, in a higher latitude, but on a lower level. 3. The Harrodsburg Springs of Kentucky, where art has contributed munificently to the comfort of inva-

lids and the amusement of the gay, but the advantages of high latitude and alpine scenery cannot be enjoyed. 4. Long-Branch, Newport, Nahant, and other marine watering and bathing places; to which may be added, 5. A trip to the Falls of of Niagara, and 6. A voyage on the St. Lawrence to Montreal and Quebec. It is not our intention to declaim against any of these resources, but to add another, which we cannot but regard as superior to either, and, in reference to certain invalids and fashionable *ennuyés*, preferable to the whole. Before proceeding, however, let us inquire, what is necessary to give effect to a sojourn in the north by the classes of persons whose tastes or infirmities demand it. In the first place, as the majority are citizens, they should, in travelling for health or recreation, seek for places and scenes that will be in contrast with their homes. The invalid cannot recover, nor the fashionable rusticate, in a crowd. 2d. The places now frequented are not so far north as to give many of the advantages of a cool climate. 3d. They offer but few novelties. 4th. They do not abound in historical associations. 5th. Although the springs of New York, Virginia and Kentucky, are valuable in several forms of chronic disease, they are useless in others and injurious in some—while the invalid seldom proceeds to drink of their waters understandingly. 6th. The amusements and dissipations in which they abound, often tempt the infirm into unhealthy indulgences. 7th. It is, we believe, an admitted truth, that, in general, but a *part* of the benefit which results from visiting mineral springs, comes from the use of their waters.

We shall now proceed to illustrate the advantages offered in the hot season, by our northern lakes; in doing which we shall draw, chiefly, upon our own notes, made on a voyage of two months, for medical observation, during the past summer.

An inspection of a map of North America will show, that this numerous and extended group lie to the north of nearly all the States; their centre of gravity, the island of Mackinac, being in the meridian which separates Ohio from Indiana, cuts through the middle of Kentucky and Tennessee, and, dividing

Georgia from Alabama, reaches the Gulf of Mexico between East and West Florida. The eye still being kept on the map, the inhabitants of the South may trace out the routes by which they can reach and embark upon the lakes. First, they who reside west of this meridian, should ascend the Mississippi and Illinois, to Chicago, on the western shore of Lake Michigan; or taking the course of the Ohio river, cross the State of Ohio by land from Cincinnati to Sandusky City, or by the canal from Portsmouth to Cleveland. Second, they who reside east of the central meridian, may cross the mountains to Pittsburgh, and make their way, either by land or water, to the town of Erie in Pennsylvania; or take the New York route to Buffalo, at the eastern end of Lake Erie, or that to Oswego on Lake Ontario.

The eastern extremity of Ontario, of which Oswego is the "seaport," lies about one degree east of Washington City—Milwalke on Lake Michigan, and Navarino at the head of Green Bay, eleven degrees west; thus the voyage of the lakes extends through twelve degrees of longitude. Sandusky City, the most northern port of Lake Erie, is in latitude about 41° 35'; Gros Cap, at the entrance into Lake Superior, in lat. 45° 29', giving a range between these parallels of four degrees. Such are the present limits, but when steamboats shall be placed on Lake Superior, which must be done at no distant time, the voyage will be extended many degrees to the west, and north to the 49th. As it is, the people of the South, from Louisiana to Carolina, may journey and sojourn, during the heats of summer, in a climate from 10° to 15° north of their own. The central lakes, Erie, Huron, and Michigan, either of which is more extensive than all the lakes of Europe taken together, are at present, and, indeed, will continue to be, the chief places of resort.

The voyage from Buffalo to Chicago is more than 1200 miles; on which the traveller is carried by long stretches to the west, the north and the south; never out of sight of land on Erie, and not long on Huron and Michigan. During the three summer months, he will seldom encounter heavy gales; but from the shallowness of Lake Erie, it becomes agitated by

gentler winds, and it is not uncommon for the invalid to experience the unwelcome benefit of a turn of "seasickness." When an occasional tempest stirs up the deeper waters of Huron or Michigan, a more formidable agitation, such as the Atlantic might not disown, arises, but this is of rare occurrence. Hence the dangers of a voyage upon the lakes are almost limited to fire, as sawyers do not exist, and rocks are scarce, while the ample "sea room" is an adequate guaranty against the fatal rencontre of boat with boat, which, on our rivers, destroys or cripples so great a number. From Buffalo to Chicago and Milwalke, there is a daily line line of steam packets, not inferior in size, strength and convenience to any in the United States; from Buffalo to Navarino, at the farther end of Green Bay, a regular packet runs every fortnight, and from the same eastern port to the *Sault St. Marie*,* 15 miles from Lake Superior, one or two boats go every summer. The whole of them touch at the island of Mackinac both going and returning. In addition to these, there are many small steamers running between all the intermediate ports of Lake Erie, Lake St. Clair and the beautful river St. Clair, up to Fort Gratiot and Port Huron in the State of Michigan, and Port Sarnia, in West Canada, all at the lower end of Lake Huron. Some of these boats also ascend the Sandusky and Maumee rivers to their Rapids, at Lower Sandusky on the former, and the towns of Maumee and Perrysburgh on the latter. From Sandusky City, from Toledo on the Maumee Bay, from Monro on the River Raisin, and from Detroit, rail road cars run from 30 to 80 miles into the interior of Ohio and Michigan; finally, at Mackinac and the *Sault* (Rapids of the St. Mary), there are fur traders' skiffs, and birch canoes, with which French *voyageurs* and *Indians* are ready, at all times, to carry travellers along or across the numerous straits which there connect together the three greatest lakes of the continent. Thus, facilities for visiting every interesting locality, in and around these Mediterranean seas, are entirely within the reach of those who may embark upon their bosom.

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* Throughout the north called—*The Soo.*

But the means of acquiring a knowledge of their area, depth, temperature, color, and other characteristics, may not always be available to those whom we hope to turn upon them; and we shall, therefore, devote a page or two to those subjects; for travel, to be beneficial, should excite the mind, as well as agitate the body.

The topographical engineers of the United States, and of New York, Ohio and Michigan, and the Historical Society of the last, have published much interesting information on these topics, which we shall use in connexion with our own limited observations.

The estimated area of Lake Ontario is 6,300 square miles of Erie 9,600—of St. Clair 360—of Huron 20,400—of Michigan, including Green Bay, 24,400—of Superior 32,000, making an aggregate of more than 93,000 square miles. From Ontario to Superior, there is a rise, which, however, is by unequal steps. The surface of the former is 232 feet above the tide water of the St. Lawrence; Erie is 333 feet higher, or 565 feet; St. Clair 6, or 571; Huron and Michigan 13, or 584; and Superior, 44 feet higher still, or 628 feet above the level of the ocean.

Of the whole, the little St. Clair is the shallowest, not averaging more than 20 feet; Erie, varying much, has no great depth any where, and does not average more than 84; Ontario has a mean depth of 500; Superior of 900, and Michigan and Huron of 1000. The deepest soundings of the whole are in the last—off the mouth of Saganaw Bay, traversed in the voyage to Mackinac, where the lead has sunk 1800 feet, more than 1200 below the level of the Atlantic Ocean, without reaching bottom.

The temperature of the lakes is diminished by latitude and depth. Thus, in reference to the former, we found a difference of 10 or 12 degrees, between the shoal water on the south side of Lake Erie and the north of Huron; and in reference to the latter, we observed, in crossing Lake Michigan from Green Bay to Mackinac, that the thermometer first sunk from 76° to 58°; then, after passing the centre of the lake, and entering the

straits of Mackinac, gradually rose to 64°. Again, on leaving the harbor of Mackinac, several days afterwards, it was 61°; in the deep waters of Lake Huron it sunk to 53°, while at the lower end of the Lake, in shallow water, it rose to 65°. We made but a single observation on the heat of the water *below* compared with that of the *surface* of the *same* spot. It was in Huron near the Island of Mackinac; where we found the surface water, and that 200 feet deep, of the same temperature—56°. Travellers, then, who cross the lakes, and wish for cool water, need not draw it from their depths, as that of the surface, at any given point, would be of the same temperature with that beneath. This uniformity results, no doubt, from the transparency of the upper lakes, by which the rays of the sun penetrate them, without meeting with solid matters in suspension to elicit their heat; it is like their passage through the cloudless atmosphere, which they do not warm.

The shallow lakes, Erie and St. Clair, are less transparent than the deeper ones above; for the winds agitate them to the bottom, which throws up a portion of its slime. The clearness of the deeper is so crystalline, that the Indians spear their fish at the depth of 30 feet; and when the voyager looks from his boat into a still greater depth, he sees the pebbly bottom, with a distinctness which deceives him into the belief that it is but a feet below him.

The color of the lakes varies with their depth and transparency. Where shallow and somewhat turbid, it is of a feeble green, inclining to a light drab; where of greater depth, and perfectly transparent, the green is much deeper, and runs into an azure teint.

The lakes have no lunar tides, but are subject to rises and fluctuations, depending on other causes. From 1819 to 1838 they gradually rose five feet three inches; after which, they fell much more rapidly than they had risen. By this rise, fruit and forest trees, some of the latter ascertained to have been more than one hundred years old. were destroyed; and consequently there had not been an equal rise within that period. This gradual and remarkable swell, is attributed by the geologists to extraordinary rains and melting snows. There is, also, a

semi-annual rise and fall of less extent, depending on the supplies which the rains and dissolving snows of spring and summer furnish; and on the congelation, in winter, of all the water which falls, with much of that already in the rivers of the north. The winds impart a daily agitation, which gives a rise and fall along all the shores, preventing stagnation and rendering the marshes which border them, less unhealthy than they otherwise would be. Lastly, the same cause, acting for some time in one direction, will pile up the waters, whence they return, when it is perfectly calm, in what are called ground swells, to overflow the shores from which they had been driven.

Some remarks on the summer climates of the lakes naturally follow those which have just been made.

When the south-west winds, which have traversed the vast plain separating the Gulf of Mexico from the lakes, reach the shores of the latter, they are necessarily dry and hot. Hence the temperature of Buffalo, Erie, Cleveland, Sandusky, Toledo, Detroit and Chicago, in the average latitude of 42°, is quite as great as their position should experience—greater, perhaps, than the traveller from Louisiana or Carolina would expect. But the duration of these winds is at no time very long, and whenever they change to any point of the compass N. of W., they bring down a fresh and cool atmosphere, to revive the constitutions of all whom they had wilted down. These breathings from the north, descend from the highlands around Lake Superior, which are nearly as elevated as the mountains of Pennsylvania and stretch off beyond the sources of the Mississippi to the Chippewyan or Rocky Mountains. In passing over that lake, with Michigan and Huron immediately south of it, the temperature of which in summer, as we have already seen, is less than 60°, these winds suffer no increase of heat, but become so charged with moisture from the extended watery surface, as to exert on the feelings of the people along the southern shores of Erie and Michigan, a most refreshing influence.

From the hour that the voyager enters Lake Huron at the

head of St. Clair River, or Michigan at Chicago, he ceases, however, to feel the *need* of such breezes from the north-west; for the latitude which he has then attained, in connexion with the great extent of the deep waters, secures to him an invigorating atmosphere, even while summer rages with a withering energy in the south. The axis of each of these lakes is nearly in the meridian, and every turn made by the wheels of his boat, carries him further into the temperate and genial climate of the upper lakes. Entering it by either of the portals just mentioned, he soon passes the latitude of 44°, and has then escaped from the region of miasms, musquitoes, congestive fevers, calomel, intermittents, ague cakes, liver diseases, jaundice, cholera morbus, dyspepsia, blue devils and duns!—on the whole of which he looks back with gay indifference, if not a feeling of good-natured contempt.

Every where on the shores of the lakes, from Ontario to Superior, if the general atmosphere be calm and clear, there is, in summer, a refreshing lake and land breeze; the former commencing in the forenoon, and continuing, with a capricious temper, through most of the day; the latter setting in at night, after the radiation from the ground has reduced its heat below that of the water. These breezes are highly acceptable to the voyager while in the lower lake region, and by no means to be despised after he reaches the upper.

But the summer climate of the lakes, is not the only source of benefit to invalids; for the agitation imparted by the boat, on voyages of several days duration, through waters which are never stagnant, and sometimes rolling, will be found among the most efficient means of restoring health, in many chronic diseases, especially those of a nervous character, such as hysteria and hypochondraism.

Another source of benefit, is the excitement imparted by the voyage to the faculty of observation. At a watering place all the features of the surrounding scenery are soon familiarized to the eye, which then merely wanders over the commingled throngs of valetudinarians, doctors, dancers, idlers, gamblers, coquettes and dandies, whence it soon returns, to inspect the

infirmities or *tedium vitæ* of its possessor; but on protracted voyages, through new and fresh regions, curiosity is stirred up to the highest pitch, and pleasantly gratified by the hourly unfolding of fresh aspects of nature; some new blending of land and lake—a group of islands different from the last—aquatic fields of wild rice and lilies—a rainbow walking on the "face of the deep"—a water spout, or a shifting series of painted clouds seen in the kaliedoscope of heaven.

But the North has attractions of a different kind, which should draw into its summer bosom those who seek health and recreation in travel. From Ontario to Michigan, the voyager passes in the midst of spots consecrated to the heart of every American; and deeply interesting to all who delight to study the history of their native land. The shores and waters of the lakes, so often reddened with the blood of those who fought and died in the cause of their country, will present to the traveller of warm and patriotic feelings, scenes which he cannot behold without an emotion under which real diseases may abate, and the imaginary be forgotten.

Should he enter the lakes at Chicago on Lake Michigan, he finds himself on the spot where the fort, bearing that name, at the outbreak of the last war with England, was surrendered to the Pottawatomie Indians, who sacked it; and then pursuing, overtook and nearly annihilated the little command of Capt. Heald—the lamented Indian spy, Capt. Wells, being among the slain.

Or should he first visit Ontario instead of Michigan, he will find that Oswego, the town of his embarkation, was once assaulted and taken; turning eastwardly, he will pass near the spot where Major Appling performed an achievement for which he was promoted; and reaching Sackett's Harbor, he may renew his admiration for the military talents of Brown, first displayed in the preservation of that place. He can then cross the lake to York, and survey the scene of one of his country's most brilliant victories, saddened, however, by the death of the accomplished and courageous Pike, cut off in the bloom of life. Advancing up the lake to Niagara, or arriving on that celebrated

river by any other route, he is at once in the midst of battle fields, where the voice of the mighty cataract was no longer heard above the din of war. Visiting the site of the British Fort George, his heart will palpitate at the recollection of the irresistible energy with which Scott and Boyd and Hindman, and their associates and men, effected its capture. Crossing to Fort Niagara, he will in imagination hear the bombardment from sunrise till dark, which threatened a destruction, that was only averted by the indefatigable perseverance and skill of Major Armistead.

Ascending the river, he will survey on the Canada shore, the rocky heights of Queenstown, stormed and taken, retaken, then lost, recovered again, but finally retained by the British, with the capture of the remnant of as brave an army as ever drew sword; he will admire anew the invincible courage of Col. Van Ranselaer the commander, of Scott and Fenwick, of Wool and Christie, of Totten and many others; he will look back, with renewed contempt, upon the twelve hundred militia, who stood as idle spectators on the American shore, refusing to cross and relieve their brethren; finally, he will not withhold a passing tribute of respect to the memory of Gen. Brock, the bravest enemy who fell on that eventful and trying day.

Rising upon the upper level of the Niagara, he will soon reach the spot where an invasion of Canada was projected, undertaken, suspended, renewed, but at last, with an ardent and willing army at his command, ingloriously abandoned by Gen. Smyth. But other and more cheering associations will rise around him when he crosses into Canada. At the sight of Fort Erie, Chippewa, and Lundy's Lane, he will recall deeds of valor which gave a new aspect to our national character; the matchless gallantry of Brown, of Ripley, of Miller, of Scott, of Jessup, of Leavenworth, of Trimble, with a long and brilliant catalogue of kindred names, will rush upon his memory; and the unconquerable courage of our soldiery, when properly disciplined, fill him with a renewed admiration of his own people and his own land.

Having listened to the deep and deathless voice of the gathered waters as they plunge below, and lingered on battle fields consecrated to his country's glory, till his soul is filled with emotions of the sublime in the natural and moral world, and images of grandeur play around him, like the sparkling bows which hover over the cataract, he may depart on his voyage, and will feel that the demons which infested his shattered nerves have been already cast out.

Embarking at Buffalo for the West, he should limit his first voyage to Erie, in Pennsylvania. Here he will find on the margin of the lofty parapet which supports the town, the ruins of the old fort *Presque Isle;* the key by which the French, from Canada, passed into the valley of the Alleghany, when they erected *Fort Du Quesne,* where Pittsburgh now stands. Advancing a few hundred yards to the east, on the margin of the same plain, he may pluck a straggling sweet briar from the spot where, for many years, reposed the bones of the gallant and restless Wayne, intetred on his return from Detroit in 1796. Close at hand he will see and may enter a blockhouse, erected in 1812, and through its port holes look out upon the winding and shallow channel, through which the enterprising Perry contrived to convey his dismantled fleet, in despite of the vigilance of Com. Barclay, who commanded the Lake. Finally, by casting his eye to the peninsula beyond, he will see rising out of the shallow waters, the sunken hull of one of the ships which soon afterwards brought down the flag of that officer, and compelled the surrender of his fleet.

Re-embarking, he will next stop at the beautiful city of Cleveland, whence he may cast his eye a few miles up the lake, and contemplate the iron bound coast on which, during a storm, the fleet of Gen. Bradstreet, carrying three thousand men, came nigh being entirely shipwrecked. As this was in 1763, it is a part of our own history. He was then on his way to relieve Detroit, for twelve months besieged by Pontiac, the most distinguished of all our Indian warriors.

Landing at Sandusky City, he will be on the spot where Bradstreet on this voyage dispersed a community of Indians

and destroyed their fields and habitations. He may here take a smaller boat, and ascend Sandusky Bay, on a voyage of 30 miles, in which, if he choose, he may stop and explore the plaster quarries of the western shore, where he will detect nature in the very act of changing secondary limestone into gypsum. But continuing his voyage, in a narrow and winding channel, through the grassy bay, which at each breath of the steamer sends up flocks of white cranes and other water fowl, he finds himself in a few hours at the town of Lower Sandusky, on the eastern edge of the celebrated black swamp. Here he stands upon the spot where the young and fearless Croghan planted the single gun, which filled the ditches of Fort Stephenson with the dead bodies of its invaders, and led to the precipitate retreat of an overwhelming British and Indian force.

Should the traveller then take the stage, on a pleasant road along the river, he will scarcely leave the town, before he passes among trees scarred with bullets in the severe contest between Maj. Ball, commanding a small squadron of horse, and an ambuscade of an equal number of Indians, in which the latter were all killed. Proceeding on his journey, he will, before night, reach the village of Upper Sandusky, for a long time the residence of the respectable tribe of Wyandot Indians, now about to emigrate to the distant west. When within three miles of this spot, he will find himself on the ill-fated battle ground of Col. Crawford, defeated and captured in 1782. Turning thence to the Tyamochtee, at the distance of a few miles, he will see upon its banks an old apple tree, indicating the site of the Delaware town, commanded by Capt. Pipe; but will in vain seek for the *precise* spot where Crawford perished at the stake, pierced from the crown of his head to the sole of his foot with firebrands.

On his return he may take the stage, and dine in Tiffin on the banks of the river, whence a railroad car, in three hours, will restore him to Sandusky City.

He may now embark for Maumee Bay, and landing in Toledo, will find, at the mouth of Swan Creek, the spot where

the Indians, when they ascended the Bay to meet Gen. Wayne, left their women and children. In another hour he can reach Maumee City, at the head of the Bay and foot of the rapids; where the first memorial of the past, which meets his eye, will be the parapets and ditches of old Fort Miami, built by the British during the Revolutionary war, and held by them after the peace of 1783. Passing up the river bank about three miles, to a spot called by the French *Presqu' Isle*, he reaches the battle ground of Wayne; and by returning along the canal a mile from the river, passes through the very woods in which the army, under the command and animated by the spirit of that impetuous veteran, on the 22d of August, 1794, drove the Indians, at the point of the bayonet, back to their wives and children at the mouth of Swan Creek. The traveller once more standing beside the ramparts of Fort Miami, will recall the daring survey made by the victorious general and his *aids de camp*, under the guns of the fort, and the spirited correspondence which followed. Turning from the contemplation of a battle so decisive, that it led to the treaty of Greenville, and to a peace with the Indians of sixteen years' duration, he will see on the opposite side of the river, adjoining Perrysburg, the earthy foundations of Fort Meigs, so vigorously besieged by Proctor and Tecumseh, and so nobly defended by our beloved Harrison; and looking down at his feet, will discover that they rest upon the batteries from which the British bombarded the fort; and then will come up the harrowing recollection, that after the cannon of those batteries were spiked, the rash and fearless sons of Kentucky, under Col. Dudley, instead of crossing to the Fort, allowed themselves to be drawn into the woods by the Indians, and slaughtered beneath the very trees, under which twenty years before the victorious army of Wayne had reposed.

Again prosecuting his voyage, the traveller in a few hours may land at the mouth of the River Raisin, and by stepping into a railroad car, be put down in a few minutes nearly opposite the straggling but celebrated village of Frenchtown. The passage of a bridge and a walk of ten minutes, will bring him

to the battle ground, where he will find guides who took part in the contest, and know every spot consecrated to the memory of some of the bravest men who ever fell in battle, or were barbarously murdered after being wounded and received as prisoners. The house on the opposite side of the river, where the Commander, Gen. Winchester, lodged apart from his heroic, but doomed little army, the night of the attack; the place where the gallant Major Madison stood, when he refused to obey the orders of his commander to surrender to the enemy; the grave of the fierce and fiery Capt. Hart, dug by the tempest when it uprooted a giant oak; lastly, the ashes of the house in which the dead and dying prisoners were consumed.

Hastening from this field of blood, the traveller re-embarks for Detroit. But if he should have taken a vessel at Sandusky, destined for that port, he will have passed between the Three Sisters, through the very waters in which the gallant Perry met the enemy and made him ours. Entering Detroit river, he finds on either hand, localities which alternately revive emotions of pride and mortification. On his right he sees Malden, at the opening of the war in the power of our army, but stupidly abandoned; on the left, Maguaga and Brownstown, where two detachments of our troops had severe, but unprofitable conflicts with superior detachments of British and Indians from Malden. In a few hours he reaches the ancient city of Detroit, and from its wharves surveys the opposite village of Sandwich, the landing place of Hull, when he invaded Canada. On entering the town, he is conducted to a tavern kept in the very house which was the head-quarters of that inglorious commander; while at the distance of a few squares, he sees magnificent edifices erected on the bastions of the fort which, regardless of the indignant remonstrances of a gallant army, he surrendered to Gen. Brock.

To relieve himself from the mortifying emotions which this spot will raise in his heart, the traveller, if he choose to make a short excursion into Canada, may take the track of Proctor's army, as he fled from Malden after Perry's decisive victory, and, standing on the banks of the Thames, contemplate the patriotism of the venerable Shelby, the heroism of Johnson,

the military genius and unostentatious courage of Harrison the commander-in-chief, the bravery of all below him, the flight of Proctor and the death of Tecumseh—second only to Pontiac in talents and a dauntless devotion to the country and interests of the Red Man.

Returning to Detroit, the traveller may indulge in new reminiscences. He can go back a hundred and forty years, when its settlement was commenced by one hundred Canadian French from Montreal under Cadillac, and trace the struggles of its infancy with the surrounding hostile tribes; then think of its transfer to England in 1760, after the downfall of Montreal; of its siege by Pontiac in 1763, and of his original and cunning stratagem for its capture—frustrated only by the gratitude of an Indian woman, which prompted her to disclose the scheme to Major Gladwin, the commander; of the surprising perseverance and resources of Pontiac during the siege, which lasted for many months; of the arrival, at length, of General Bradstreet, with a numerous army; and of the retirement of Pontiac to the Illinois river, where he was soon after assassinated by an Indian. But the traveller's recollections will not be exhausted by these events; he will remember, that from the commencement of our revolutionary war, till the Indian treaty of Greenville, a period of 20 years, Detroit was the seat of that British influence which instigated and encouraged the Indians to make war upon the scattered settlements of the Ohio river, from its sources to its mouth; that Col. Byrd with six pieces of artillery actually penetrated into the heart of Kentucky; and that the ablest of her many heroic defenders, Gen. George Rogers Clarke, actually captured Hamilton, the Governor of Detroit, at Vincennes on the banks of the Wabash.

Leaving this ancient capital of the lakes, the voyager embarks for the Island of Mackinac, and standing on its summit, thinks over the eventful past. In front, reposing on a rock, are the battlements of Fort Mackinac; to his right, the shores on which the British landed, and at the dawn of day demanded and obtained a surrender of the fort, garrisoned only by a handful of men, ignorant of the declaration of war; underneath

and around his feet he will see the foundations of another fortress, built by the conquerors; descending to the west, in a mile he arrives at the field where Col. Croghan, after effecting a debarkation for the purpose of reconquering the island, was repulsed with the loss of the brave and accomplished Major Holmes.

Returning to the village of Mackinac, he finds, in his way, a projecting rock, once the roof of an alcove or cavern, which, his guide will tell him, is called the Cave of Sculls, and then his memory will turn upon remoter events. As he emerges from the woods, and casts his eye across the strait, to the main land of Michigan, he will see, dim in the distance, the site of old Fort Mackinac, besieged, and taken by stratagem in the Pontiac war of 1763; he will recollect the story of its massacre; the thrilling incidents of Henry's captivity, and his concealment in the Cave of Sculls, under the vigilance of a generous and faithful Indian, who had adopted him as a brother.

Such are some of the historical associations connected with a voyage upon the lakes; and where else in the Union can the invalid and the patriot roam, to find localities so opulent in varied and affecting recollections—so accessible—so arranged upon the thread of travel! We may fearlessly affirm, that in this respect, the lakes of the north take precedence over any other region of our beloved country. Their deeply wooded shores yield a bountiful harvest of facts to the historian; while their green waters reflect images of glory, sadness and shame, which the poet and orator will embody and bequeath to posterity.

Exciting and curative as it must be, for the valetudinarian of taste and intelligence, to pass a summer amidst scenes which can thus touch his feelings, and beguile him into forgetfulness of his sufferings, whether real or imaginary, he has still other sources of profitable recreation, in the voyage we are gratuitously prescribing; and in pointing them out, we shall designate the places on the lakes, at which he should repose and look around him.

The towns of Buffalo, Erie, Painesville, Cleveland, San-

dusky, Manhattan, Toledo, Maumee, Perrysburg, and Monro, afford good accommodations and excellent society, but as we cannot recommend a *protracted* sojourn on the shores of Lake Erie, in the heat of Summer, we shall leave them, to speak of places farther north.

Detroit, lying above the latitude of 42°, offers to the people of the South a salutary change of climate, as well as a place of elegant repose after the fatigue and excitement of a long journey; and being the key to the upper lakes, is a suitable spot on which to fit out for more distant points. Its society, in cultivation and hospitality, is among the best in the Union. It is the permanent residence of the urbane old soldier, Brigadier General Brady—a lieutenant in Wayne's victory, a wounded colonel in the bloody battle of Niagara, and now the commander of our northern posts; also, of the Stuarts, uncle and nephew, who figure so largely in Washington Irving's Astoria—the latter, at present the General Superintendent of Indian Affairs for the north; of H. R. Schoolcraft, extensively known as a writer on the Indians; of Dr. Douglas Houghton, the indefatigable Geologist of Michigan; and of Col. Henry Whiting, who has dignified his profession by the cultivation of letters. In addition to these, the city is the residence of the Governor and officers of Michigan; of the Federal District Judge; of the Episcopal and Roman Bishops of the State; and of many highly intelligent gentlemen, who have been attached to the army, and the Indian Department, or engaged in the Indian trade. Finally, as she resides in the neighborhood, the traveller may have the good fortune to meet with Mrs. Clavers, whose vivid pictures of life in Michigan, foreshow that she is destined to become one of our most popular writers.

Detroit, once a French village, is now so thoroughly Americanized, that it is chiefly among the *names* of its inhabitants, we find the lingering evidences of its origin and early condition; but above and below the city, on either bank of the river, the habitations and wind-mills of the Canadian French, con-

stitute a striking feature of the landscape, and contribute to that variety, which imparts efficacy to travel, undertaken either for health or recreation.

Of the two railroads from Detroit, one may be travelled 18 miles—the other 80. The latter runs nearly west, and makes the first link in the route to Lake Michigan. Thus, the invalid traveller, in a few hours, may place himself in the interior of the State, and wander among its miniature lakes. Stopping in Ann Arbor on his return, he may visit the new State University, with its magnificent museum of the natural productions of Michigan, from Erie to the distant shoresof Lake Superior, arranged by the State Geologist.

The town of Port Huron, at the lower end of the lake bearing that name, may be advantageously visited for two or three days. It is built on a low *dune*, and through its centre flows Black River, whose waters have the color of diluted ink. A mile above, he can visit and inspect Fort Gratiot. On the opposite side of the strait, he may spend a day, in the vicinity of Port Sarnia, among a permanent band of Chippewa Indians, rising into civilization under the efforts of a Methodist missionary, aided by the Canadian government.

Chicago and Milwalke, on the western shore of Lake Michigan, in lat. 42°–3°, although, like the towns of Lake Erie, too far south for the summer *residence* of travellers, present the phenomena of a refined society, and elegant towns built, like the ancient Tadmor, in the wilderness; but the facilities for reaching them are very different, as a daily line of commodious steamers communicates with both; and affords an opportunity for those extended voyages, which give to the lakes of the north a part of their pre-eminence over all other places of resort on the continent.

Green Bay, the name of a beautiful gulf of Lake Michigan, is also the name of a township at its head, within which are Fort Howard, not now garrisoned, and the beautiful, young,

out not growing villages of Navarino and Astor, adjoining each other. Between them and the fort, all built on low *dunes*, Fox River, having its origin in the little lake Winnebago, enters the Bay. Around its mouth, in all directions, are grassy marshes communicating with the Bay; but the traveller need not feel apprehension at leaving his boat and awaiting her return, in a fortnight; as the fevers of the south are almost unknown at this spot, the latitude of which is nearly 45°. The chief motives of sojourn here, are its distance to the north west, the wildness of the surrounding country, and the proximity of the Oneida and Menomonee Indians; the former, emigrants from New York, and considerably advanced in civilization—the latter, in a state of original barbarism. Families of both may be seen daily in the villages, where they bring their manufactures, of which the traveller may readily obtain many curious and amusing specimens. But the island scenery of Lake Michigan and Green Bay, is perhaps the greatest attraction to this distant voyage. It is not easy to conceive of any thing more beautiful. Green waters sending up green islands, relieved by white girdles of lime-rock interposed between the waves and trees, are objects which the most querulous dyspeptic could not behold with indifference; nor the traveller of taste, look upon without rapture.

The Sault, or Rapids of the St. Mary, is a still more interesting place of sojourn than the last. By a circuitous navigation, it is 90 miles above Mackinac and 15 below Lake Superior. The voyage, chiefly through straits winding among innumerable islands, is attended with some difficulties, while there is no emigration and not much travel in that direction, and hence but few steamboats go thither. Schooners, however, visit the *Sault*, and the Indians and Canadian French ply between it and Mackinac in skiffs and birch canoes. As we ascend the St. Mary, hills and ridges of primary rock, rise to such a height *compared to the shores below*, as to present something of mountain scenery.

As the *Sault* has the latitude of forty-six and a half degrees, it is the most northern summer residence which our country

affords; and corresponds with Tampa Bay, as a place of resort in the winter. It was settled, as a Canadian fur trading establishment, nearly two centuries ago; and a long experience proves, that its summer climate is still one of the most healthy on the continent. It is still *but* a trading establishment, protected by Fort Brady, and inhabited by a mixed population of Americans, French and Indians. The British North West Company have a factory on the opposite shore of the river. An Episcopal Missionary keeps a school for Indian children on the same side, and there is another in the village supported by the Methodist Church. Wild and distant, then, as this place may seem to the fashionable valetudinarian, he will perceive, that on going thither he will not pass beyond the companionship of clergymen, teachers, officers, merchants and other members of civilized society; while every hour in the day will furnish him with groups of Chippewas; and every walk bring him to some one of their conical lodges, constructed of poles and flag mats, or cypress bark.. He will also be in the midst of another class of men, almost as different from himself as the wildest Chippewas. The Canadian *voyageurs*, born and reared on the shores of the lakes, are a peculiar race, and add much to the picturesque aspect which society here presents. Equally animated and at ease, both on land and water, in the villages and the wilderness, among civilized men and the wildest Indians, they are the cosmopolites of the North, and the backwoodsmen of Paris; for whether encamped with Chippewas on the banks of the St. Mary, or engaged in transporting the travellers of the South to its source in Lake Superior, they display not a little of the gaiety of heart, and courtesy of manners, which characterize the French metropolis.

The canoe or skiff voyage up the St. Mary's, from the *Sault* to Lake Superior at *Gros Cap*, on the Canada side, is the most interesting of all the shorter excursions in the North. The traveller *may* go and return the same day, but he is too much hurried for accurate observation; and loses, moreover, the pleasure of encamping *a la Sauvage*. To enter a tent, or

to *bivouac* on a sand bank, beneath pine trees, among grass and flowers, uninfested with gnats, musquitoes or snakes, and lodge for a night on a bed of fern, is a luxury of itself; but when we add the music of the waters at his feet, the solemn stillness of neighboring woods, the mingled merriment of the *voyageurs* and Chippewas, their clouds of tobacco smoke, and the draughts of hot tea made from the leaves of an adjoining bush, the hypochondriac rises in the morning from a delicious *midsummer-night's dream*, and goes on his way rejoicing.

In making this excursion, the disciple of good old Isaac Walton may watch the writhings of his worm in the deep and pellucid waters of the lake; the geologist break off specimens of wacke and old red sandstone from its banks; the *virtuoso* pick up shells and cornelians on the beach below; the botanist enrich his herbarium with flowers, the painter his portfolio with original sketches, and the lovers of nature at large, their imaginations with the wild and beautiful.

On returning, they may descend the *Sault* or Rapids, when, for nearly a mile, their little barque, as if by instinct, will rapidly pick its way through dashing currents and whirling eddies; while snatches of song by the Canadian boatmen, and the startling yells of the Chippewa Indians, will raise a chorus to the tumult of the waters, which their friends below as loudly echo back.

At the *Sault* resides Mrs. Johnson, the intelligent Indian mother-in-law of the two Schoolcrafts. The elder we have already mentioned; the younger, for seventeen years associated with the Chippewas, lives near her. This place is, also, the residence of John Tanner, captured more than fifty years ago, on the banks of the Ohio, in Boone County, Kentucky, and introduced to the reading public by Dr. James' Narrative. But a different inhabitant, of more interest than either to the dyspeptic and the gourmand, is the celebrated WHITE FISH, which deserves to be called by its classical name, *Coregonus albus*, which liberally translated signifies—food of the Nymphs. Its flesh, which is the cold and clear waters of the lake organganized and imbued with life, is liable but to this objection,

that he who tastes it once, will thenceforth be unable to relish that of any other fish.

The Island of Mackinac is the last, and of the whole, the most important summer residence to which we can direct the attention of the infirm and the fashionable. True, it has no mineral springs; but living streams of pure water, cooled down to the temperature of 44°, gushing from its lime-rock precipices, and an atmosphere never sultry or malarious, supersede all necessity for nauseating solutioas of iron, sulphur and Epsom salts. An ague, contracted below, has been known to cease even before the patient had set his foot on the island, as a bad cold evaporates under the warm sun on a voyage to Cuba. Its rocky, though not infertile surface, presents but few decomposable matters, and its summer heats are never great enough to convert those few into miasms.

Situated in the western extremity of Huron, within view of the straits which connect that lake with Michigan, and almost in sight, if forest did not interpose, of the portals of Lake Superior, this celebrated island has long been, as it must continue to be, the capital of the upper lakes. The steamboats which visit the Rapids of the St. Mary and Green Bay, not less than the daily line from Buffalo to Milwalke and Chicago, are found in its harbor; and the time cannot be remote, when a small packet will ply regularly between it and the first. By these boats the luxuries of the South, brought fresh and succulent as when first gathered, are supplied every day. But the potatoes of the island, rivalling those of the banks of the Shannon, and the white fish and trout of the surrounding waters, yielding only to those of Lake Superior, render all foreign delicacies superfluous. We must caution the gourmand, however, against the excessive use of trout (*salmo amethystes*), which are said to produce drowsiness; for he who visits Mackinac, should sleep but little, lest some scene of interest may pass away unobserved.

The society of the town and post is every way equal to any reasonable expectation. There are three plain but comforta-

ble houses of entertainment for strangers; and an increase in the latter would be followed by an immediate extension of accommodations, perhaps by the erection of a summer boarding house on the high eastern cliff of the island, which would be one of the most attractive in the United States. Several of the old French and mestizoes, are intelligent and courteous; there a number of traders and other gentlemen, whose long and extended intercourse with the natives, has qualified them to impart much curious and interesting information, and the sub-Indian agent, and the officers and chaplain of the army, are hospitable and gentlemanly; while last but not least, the female society is superior to what might be anticipated, in a town so remote as to connect savage and civilized men in their daily business.

The waters around the island are so narrow and tranquil, that many excursions of pleasure, in skiffs and canoes, may be performed with safety. Several other islands lie in view, and the coasts of the upper and lower peninsulas of Michigan, stretch off in four blue and beautiful curves. In a single hour Point St. Ignace may be reached, and in twice that time the traveller may stand on the site of old Fort Mackinac, already mentioned. These opposing shores constitute the portals of Lake Michigan. Passing through them, the enterprising traveller may perform a voyage on the coast of Michigan, to the Catholic, Ottowa villages of *L'Arbre Croche* and the *Petite Traverse;* and continuing it, reach the Chippewas of the *Grande Traverse* Bay, where he will find a Presbyterian Mission. At both settlements, he will see much of primitive Indian life, in the midst of a half civilized and grotesque population.

But should he *remain* on Mackinac, he will not be without interesting sights, for those which do not belong to it will come. The chief place of *Indian* trade for the north, and a stopping port for all who wander from lake to lake, it is the daily resort of Ottowas, Chippewas, and Pottawatomies, the two former, however, by far the most numerous. Here they bring their mokuks of maple sugar, their furs and dressed skins, their flag

mats, embellished moccasins, feather-fans, and white birch boxes, baskets, and miniature canoes, ornamented with dyed porcupine quills. In addition to the few who are at all times visiting the island, a large number from the shores and islands of Lake Michigan and Green Bay, every summer congregate here, on their way to the Great Manitoulin Island, in the northern part of Lake Huron, to receive presents from the British Government for having fought against us in the war of 1812. A hundred conical lodges, on the beach in front of the village, with as many birch canoes drawn out of the water, may be seen at a single stroke of the eye, presenting with the fort above, the churches of the village below, and the steamboats and schooners of the harbor, a contrast of wonderful novelty and interest. But when they depart under a west wind, each having a square sail, and are seen from the eastern summit of the island, at every distance, the fleet of Lilliputian barques, might not inappropriately be likened to a scattered flock of gigantic swans, gracefully moving through the azure waters.

In conclusion, we must devote a page to the natural scenery of the island. Its entire circumference does not exceed ten miles. Seen as we approach from the east, it presents a mural precipice, of grey secondary limestone, rising 150 feet out of the green waters, and decorated on its brow with maples, oaks and evergreens. Over a chasm in the verge of this cliff, is a natural bridge, so narrow and elevated, that one of the exploits of the daring visiter, is to walk upon it. At a short distance in its rear, stands a conical rock, whose pinnacle overtops many of the forest trees, in the midst of which it has stood in solitary and undecaying dignity, while they, generation after generation, have mingled with the soil. On the western slopes of the island, there is an immense number of primitive boulders, from the granitic mountains beyond Lake Superior; lastly, on the very summit of the island, the naturalist may collect organic remains, and the curious peel white birch bark, on which, should they lack paper, they may write their notes, or correspond with their distant friends.

In a recess on the south-eastern side of the island, but a few feet above the surface of the lake, stands the grotesque village of Mackinac; where, side by side, are Canadian, cypress thatched cabins, and modern frames erected by our own people. On the cliff which overhangs it, sits Fort Mackinac, with its bristling cannon, and white-washed battlements. Half a mile in the rear is another plateau 75 feet higher, the site of old Fort Holmes, which we have already visited. From this summit, elevated far above all that surrounds it, the panorama is such as would justify the epithet to Mackinac, of Queen of the Isles. To the west, are the indented shores of the upper peninsula of Michigan; to the south, those of the lower, presenting in the interior, a distant and smoky line of elevated table land; up the straits, green islets may be seen peeping above the waters; directly in front of the harbor, Round Island forms a beautiful foreground; while the larger *Bois Blanc*, with its lighthouse, stretches off to the east; to the north are other islands at varying distances, which complete the archipelago.

When the observer directs his eye upon the waters more than the land, and the day is fair with moderate wind, he finds the surface as variable in its tints, as if clothed in a robe of changeable silk. Green and blue are the governing hues, but they flow into each other with such facility and frequency, that while still contemplating a particular spot, it seems, as if by magic, transformed into another.

But these mid-day beauties vanish before those of the setting sun, when the boundless horizon of lake and land seems girt around with a fiery girdle of clouds; and the brilliant drapery of the skies paints itself upon the face of the waters. Brief as they are beautiful, these evening glories, like spirits of the air, quickly pass away; and the grey mantle of night warns the beholder to depart for the village, while he may yet make his way along a narrow and rocky path, beset with tufts of prickly juniper. Having refreshed himself for an hour, he may stroll out upon the beach, and listen to the serenade of the waters. Wave after wave will break at his feet, over the white pebbles, and return as limpid as it came. Up the straits he will see the

evening star dancing on the ruffled surface, and the lagging schooner flapping its loose sails in the fitful land breeze; while the milky way—Death's Path of the Red Man, will dimly appear in the waters before him. Behind, in the street, a lively group of Canadian French, of every shade of color between white and red, will gossip and shrug their shoulders; on one side, he will hear the uproar of a lodge of drunken Chippewas, with the screams of women and children, and the cackling of frightened hens; on the other will see, the sober and listless Ottowa, sitting in silent vacancy of thought, on the upturned keel of his birch canoe, his wife within the tent spreading cypress bark and flag mats upon the gravel, as lodgings for the night, while half a dozen children loll or play about the door, and as many half starved dogs curl up among them. Surrounded by such scenes, the traveller begins to realize that he is a stranger; when suddenly a new phenomenon appears and fixes the conviction. Every object becomes more visible, and, raising his eyes, he beholds the heavens illuminated with an aurora borealis, where he reads in fantastic characters of light, that he is, indeed, a sojourner in a strange land, and has wandered far from his friends and home in the sunny regions of the South.

Nov. 1842.

Art. II.—*Remarks on the Diseases that appeared in Rutherford County, Tennessee, during the year* 1841, *and the winter of* 1841–2; *being a Report read before the Medical Society of Tennessee*, at its annual meeting in May 1842. By John W. Richardson, M. D.

Amongst the diseases above alluded to, intermitting fever should be first mentioned, because it exists almost continually with us. I have nothing new to communicate on intermittents.

Measles prevailed extensively during the early part of 1841, attended with more arterial excitement than I have usually

observed, though nothing of importance transpired in relation to this disease.

Whooping-cough has prevailed for twelve months, attended with its usual characteristics.

Parotitis has also been in my neighborhood for a year, and is still prevailing. It has progressed remarkably slow—remaining in some families for four, and even six months. It is attended with more fever, more cerebral disturbance, and is more often followed by hernia humoralis than I have usually seen it. Some interesting and hitherto unobserved facts were noticed in connexion with this disease. I was requested, last spring, to visit a neighbor, whom I found suffering greatly from pain in the head and swollen testes. No thought was entertained of mumps, as the disease was not in the vicinity at this time. He was treated by venesections, saline purgatives, in which some tartar emetic was dissolved, leeching and warm fomentations to the testes. He soon recovered. Shortly afterwards I saw another case of precisely the same character; and in a few days a third, which was attributed to riding on a wet saddle. The two last cases were treated like the first, and with the same result. In a few days from the attack of the two last patients, parotitis, in its usual form, appeared in each of their families, and two negro men, in one family, were similarly affected with their master. No doubt was then entertained by me but that this inflammation and swelling of the testes was produced by the specific cause of mumps. From that time to the present, parotitis has prevailed in my neighborhood, and many cases have occurred to verify this conclusion. Not one of the individuals who had inflamed testes primarily has had parotitis: but many who had parotitis, had hernia humoralis as a secondary affection. This is common upon exposure. The same treatment succeeded in relieving this secondary affection. Will any of the persons who had testitis primarily, ever have parotitis? I think not. We frequently meet with cases of testitis (I hate the term *hernia humoralis*) in our practice. Scarcely any disease produces so much pain and distress to a man. And here let me remark, that the best ano-

dyne I ever used for it, is just as much "tartar water" as the stomach will retain.

No case of metastasis from the parotids to the mammæ in the female has been observed, though it is frequently spoken of by medical authors; but in several cases under my care the uterus was excited to great action. In pregnant females abortion and miscarriage were produced, and if the translation or sympathetic action took place at the menstrual period, the flow was abundant. There has been no case of death from the disease (parotitis) to my knowledge. In the general it required only rest, light diet, and perhaps one purgative. In many cases nothing was required. The disease has visited almost every family in my region, and four-fifths of the people have had it.

Our summer and autumnal diseases were of the usual character—intermitting and remitting fevers, with cholera infantum. They presented no unusual appearance and yielded to the ordinary treatment. Late in the fall Dr. Murdock and myself attended some rather curious cases of fever, which I will briefly relate. They were all in the same family. One of the cases occurred sooner than the other three, but they were all characterised by the same symptoms, with some slight and unimportant exceptions. The patients were black girls—the oldest nearly grown, and the youngest three years of age. They were attacked with great pain in the forehead, followed by stupor, deficient capillary action, cold extremities, torpid bowels, and a constant disposition to closed eye lids. Reaction was limited and feeble. One of the girls (the oldest) was deaf in one ear, and heard only very loud sounds with the other. She complained of her head, back, and of a choking at the top of the sternum; would frequently rave and talk vociferously when she was aroused. One of the others was entirely deaf for more than a week; pulse quick, frequent and small, occasionally tense; tongue covered with a thick, yellowish-white coat. The duration of their sickness from the time of attack until dismissed from medical attendance, was four weeks. These cases were treated with stimulating diaph-

oretics, cupping, mercurial purges, blisters and quinine. They all recovered, though convalescence was remarkably slow.

In the month of November an epidemic influenza commenced. Many cases were alarming, so extensive and severe were the bronchial inflammation and pain in the head. These cases were treated with a general bleeding, cupping over the chest, mild purgatives, tartar water, mucilaginous drinks, warm pediluvia, with Dover's powder at night. This disease prevailed through the months of November, December, January, and to some extent in February. The cough was generally very distressing, and frequently remained for a long time after all the other symptoms had subsided. The syrup of squills, blisters and tartar ointment, always avoiding exposure, relieved it.

During the winter we also had some cases of pleurisy, a good many cases of pneumonia, and perhaps a combination of the two, attended with considerable cerebral disturbance. They were treated pretty much in the same manner as influenza, only requiring the lancet more freely, a large dose of calomel and ipecacuanha immediately afterwards, and the constant use of antimonials. Blisters were generally used after active depletion, and with the happiest result.

Cynanche tonsillaris also visited us last winter, attended with high fever and great head-ache. There were not many cases. A free bleeding, followed by the usual antiphlogistic course, and scarification of the tonsils relieved it. If these means were neglected, the tonsils were apt to ulcerate, which required the use of stimulating astringent gargles. Thus in the brief space of twelve months, we have had, in addition to our usual summer and autumnal diseases, measles, mumps, whooping-cough, epidemic influenza, pneumonia, cynanche tonsillaris, and a good many cases of pleurisy. All these diseases, with the exception of measles, prevailed to a greater or less extent through the months of December, January and February; and the mumps and whooping-cough are still with us. We succeeded pretty well in the treatment of these diseases; some cases were violent and required active treatment

and good nursing; but to these they generally yielded, and nothing very unusual was discovered in them.

The month of January was a warm and wet month, and towards its close we were visited by an epidemic more unmanageable than any of the former, and more *fatal* than the whole of them combined. The first case that I saw or heard of, occurred on the 24th of January. The disease was characterized by the following symptoms: a chill, or chilliness ushered in the attack; great pain in the head; nausea or vomiting in some cases; more frequently there was neither. In a short time reaction came on (when it came at all), and those who lived to experience it, became either stupid, comatose, or quite vociferous. The great majority became restless, talkative, boisterous, calling those to whom they were most attached in health, pulling and scratching the bed clothes, sometimes biting their finger nails, and occasionally screaming, as though they were frightened. Others lay perfectly stupid; some muttering incoherently, and when aroused, would answer a question rationally, if addressed in a sharp, quick, tone, and would immediately commence writhing and twisting and talking incoherently again. Those who died before any reaction was established, would roll from one side to the other, and toss about in every possible manner, apparently insensible to every thing around them; and even they, when aroused, would sometimes answer rationally.

There were some appearances on the skin of white children that could not be observed in the negro. When the excitement came on, after the chill, a few very red specks were frequently noticed, scattered promiscuously over the forehead, breast, and arms. This eruption was very distinct, and more general in a mulatto girl, than any case I saw. Another eruption of a different character was noted in three patients. Clusters of vesicles (resembling those in shingles) appeared on the lower lip of one patient, extending from the epithelium over the chin. A group of some fifteen or twenty were observed on the cheek of one patient, and as many on the trachea of another.

The eruption first spoken of generally disappeared in a few days without any desquamation, except in one case, where the pimples became larger, more prominent, and filled with a sebaceous looking matter.

Partial deafness in almost every case—blindness, total in one case, almost entire in the second, and partial in a third. In a fourth case there was blindness, I am quite confident, but I could discover no cause for it, as there were no unnatural appearances about the eyes, except a slight contortion. In one of these cases (the first) several whitish flocculi were observed floating, apparently, in the aqueous humor, which continued to increase in size until vision was entirely obstructed. This patient died on the fourth day. In the second case there was the appearance of a beautiful cataract in each eye. The pupils were smartly dilated, and the opacity was not quite large enough to fill the pupil, consequently vision was not entirely obstructed, for I discovered the little fellow could see and distinguish bright objects in an oblique direction. This patient recovered. His eye-sight gradually improved, all morbid appearances were slowly removed, and he sees now as well as he ever did, although he *squints* a little.

In another case, the conjunctiva of the right eye became very red and highly inflamed some two or three days after the patient was seized with the epidemic. It caused him much pain, and vision was entirely destroyed. After the inflammation subsided his eye was minutely examined, and no unnatural appearance was discovered, except a shining yellowness of the crystalline lens or its capsule.

The bowels of all were generally torpid—all the secretions seemed to be arrested, or retained. The urine was frequently not discharged for twenty-four or thirty-six hours after the attack. In one case, the arms of a little girl were paralyzed for several days, and she screamed violently whenever they were moved, or even touched. She recovered. Except the paralysis, this was not a bad case.

In nearly all the bad cases, the head was drawn back on the shoulders, and the whole spine, from the head to the sa-

crum, was bent like a well strung bow. So great was the extension of the body that many patients could not lie on the back at all. I never saw such a contraction of the spinal muscles, except in the species of tetanus called opisthotonos. The tongue was generally clean at the attack, but after the first excitement it became covered with a short, thick, white coat.

I will relate, briefly, the history of three cases, which will serve as indices to the rest.

Thomas Hardeman, a little boy, aged 12 years, complained of headache on Monday evening. When supper was announced, he was found lying on the bed, and asleep. He was taken up and carried in a gentleman's arms to another room, when he complained of sickness, and vomited freely. Reaction came on directly, and in a short time he perspired profusely.

At sunrise, I was requested to visit him, but did not, on account of the condition of my little daughter, who also had a similar attack, and who died on the 16th day, as a consequence, I believe, of the treatment.

Dr. Starnes, one of my late pupils, who happened to be at my house at this time, visited this patient for me. The Dr. says he found him, immediately after sunrise, cold, almost pulseless, surface of the body covered with large purple spots, rolling on the bed, comatose, and rejecting every thing he swallowed. When aroused, he answered rationally. The most powerful stimulants, hot applications and frictions, sinapisms, a blistered stomach, camphorated ether, an attempt to bleed, &c., all failed to produce reaction, and the little fellow soon expired.

Harriet, a servant girl of my neighbor, J. Sykes, Esq., about the same age, slept quietly through the night, and arose well, to all appearance, in the morning. She took the infant from her mistress, and while opening the door to leave the room, fell to the floor with a groan. Upon examination by her master, he says he found her cramped all over, and speechless Her extremities became cold directly, when she

was placed in a hot bath, and an emetic given, according to previous advice. Just after this, and while the emetic was still operating, I saw her. Reaction was being slowly established; it was assisted to a point deemed sufficient, when she was cupped freely between the shoulders, and afterwards bled from the temples. A large dose of calomel was given, and another left for her at night with Dover's powder; spts. mindereri to be given as a febrifuge. The medicine failed to operate on her bowels, and enemata were employed. After a while they succeeded.

She spent a restless night, and I found her next morning in pretty nearly the same condition as when I left her, twisting and turning on her bed, calling her master, and talking incoherently. A blister was applied to the nape of the neck, and very nearly the same course of treatment pursued as will be related in the next case. She died on the fourth day.

Mary, aged 13 years, servant girl of Capt. J. Smith, had a chill, and severe headache. An emetic was given. Reaction came on, attended with great stupor. She was bled to a pint and immediately afterwards I saw her. She had also taken some purgative pills, but they had not operated. She was resting quietly, very stupid, though answering rationally when aroused. Cupped her freely on the back of the neck, applied a blister, and gave 20 grs. calomel. By the next day the medicine had operated several times, and the last discharge was very thin and yellow. Pulse small and quick. Gave a dose of laudanum (20 drops). She complained greatly of her head and back; placed her in a barrel of hot water, and her pulse improved. Gave her 10 grains of blue mass, and 8 grains of Dover's powders, to be taken at night.

The next day her head was drawn back, and the spine curved like an arch.* Pulse as yesterday. Placed her in the bath again—gave a cordial. Discharges from her bowels frequent and small, though of a tolerably good character. Skin dry; tongue covered with a thick, short, white coat. Gave

* The girl Harriet was in the same condition.

her spts. mindereri every hour, and blue mass and Dover's powder at night.

The next day (20th February) found her condition about the same as previously. Exchanged the spts. mindereri for camphorated ammonia, as the patient seemed more feeble. Applied half a dozen dry cups on the spine. Continued blue mass and Dover's powder. Directed the spine to be well rubbed with brandy, cayenne pepper and camphor, and the dry cupping to be repeated at night—to take oil in the morning if the bowels are not moved.

21. Visited patient at 1 o'clock, P. M. Oil had been taken and had operated. Same treatment continued and the bath repeated.

22. Condition, perhaps, a little improved. Same treatment continued.

23. Patient seemed evidently better, and from this time she slowly recovered.

The respiration, in nearly all the cases which I saw, was free; frequently, remarkably soft and easy. In a few cases there was sighing occasionally. There was no cough—no signs of pulmonary engorgement, nor was there any evidence of primary disease any where save in the brain, spinal cord, or ganglionic system. And in proportion as one or the other of these systems was affected, so was the predominance of the symptoms peculiar to it. Was the primary lesion in the brain? There was coma, at times listlessness, pale and vacant countenance, and if the patient was elevated, nausea and vomiting were induced. Was the spinal cord the first to suffer? A tetanic spasm of the cervical and spinal muscles, great pain in the neck and along the whole course of the spine, were the predominant symptoms. Was the ganglionic system attacked? The bowels were torpid, all the secretions suppressed, and medicines had but little effect. The greater number of my cases was a complication of the spinal and ganglionic forms. Severe pain in the head or back was universally complained of by all those who could give any account of their sufferings. Many cases of influenza, and some of pneumonia and tonsil-

itis "passed off" for this singular disease. But none of the cases which I saw had a single symptom peculiar to the epidemic except pain in the head. I heard of many cases and saw some of those that were called "this head disease," but they were not. The attack came on with a chill, followed by considerable reaction. The determination was to the mucous surface of the lungs, inducing a bronchitis; or to the parenchymatous portion, when a pneumonia was the consequence, and the cough succeeded the reaction in both instances.

The only description, which I have seen, of any form of disease that assimilates this epidemic, appeared in the Gazette Medicale and subsequently in the Medico-Chirurgical Review (No. 86, for 1841). The disease prevailed at Versailles, Rochfort, Metz, and at Strasbourg. The history of the epidemic, as it appeared at Strasbourg, is given by Dr. Wunschendorff; and as it may not be in the hands of all those who read this article, I have concluded to copy it from the Review.

"The *symptoms* of this epidemic meningitis were usually sudden headache, vertigo and vomiting; then delirium, convulsions, and tetanic spasms of the jaw and trunk; and lastly coma, paralysis of the sphincter and other parts of the body, and the usual phenomena of malignant typhus. The bowels were in most cases constipated at first and afterwards became relaxed. The pulse was often remarkably slow at the commencement and in the early stage of the disease. In the majority of cases an herpetic eruption of the lips made its appearance.

According to the predominance of certain symptoms, the Dr. describes an inflammatory, a neuralgic, a convulsive, a delirious, and a comatose form of disease. But he has not been able to point out a connection between these different forms and any special modification of the necroscopic phenomena: on the contrary, the morbid lesions discovered on dissection were almost always nearly the same. The *course* of the disease was very irregular; in many cases it exploded with symptoms of high excitement, under which the patient sunk, or which were succeeded by torpor and various other phenomena. The duration of the disease also varied much in different cases; in some it proved fatal in a few days, or even in a few hours, while in others it was lengthened, after the first

violence passed away, to one or several months; before terminating in death, or in a slow and painful convalescence.

The diagnosis was occasionally difficult, the disease assuming the character either of malignant fever, follicular enteritis, or apoplexy. The prognosis was necessarily unfavorable. One half of the patients died; but this mortality is common to this epidemic with sporadic meningitis, which usually proves fatal in two out of every three cases. In the civil hospital at Strasbourg, 21 patients died out of 40 that were admitted; and in the military hospital the mortality was still greater—104 out of 176.

The *treatment* followed was entirely antiphlogistic. The patients were usually bled several times, and at short intervals, varying according to the severity of the symptoms during the early stage. Leeches were applied in great numbers to the temples and behind the ears, and cupping also along the spine was practised freely. Cold lotions were applied to the head, and in some cases mercurial ointment was smeared on the shaved scalp.

Having lost several patients, to whom calomel in large doses was administered, and as there were usually abdominal symptoms present, Professor Forget subsequently discontinued the use of intestinal derivatives, and trusted altogether to mild aperient enemata.

In the more advanced stage of the disease much reliance was placed on cutaneous irritants. At this period, opium in the form of syrup seemed decidedly useful in mitigating the headache and delirium. All stimulant and tonic medicines were hurtful, except during convalescence."

Such is the history of this epidemic. No *cause* has been assigned for it. What relationship it bears to the epidemic that constitutes the principal part of this essay, is left for the decision of those who may examine both as here related. That there are some striking similarities will be discovered by the superficial observer.

No one, so far as I am informed, has ventured to give "the epidemic" either a name or habitation. Nor do I believe any one has promulged, even what he might *suppose* to be the cause. The cause and pathology of the disease are subjects of interest and importance; and as we have no knowledge of either, I will give you the *best faith* I have upon the question, and await further revelation.

The last winter was remarkable for its mildness. The month of January was wet and very warm. Inflammatory diseases had been, and were still prevailing up to the appearance of the epidemic. Why so great a disposition to cerebral, cerebro-spinal and nervous lesions should exist at this time, we are unable to state. For, even if we suppose that the epidemic was not the effect of a peculiar and specific cause, but was only produced by the general cause or causes then acting in the production of the inflammatory diseases which existed at the time of its appearance, still we must conclude that something more was necessary in order to the developement of such peculiar symptoms. Whether the wet and warm month of January was sufficient to produce these peculiarities, is also a question which deserves attention; or whether a cause independent of those then acting, and one *sui generis*, is another question. I am inclined to believe that the cause was peculiar, and produced in every case, where its effects were cognizable, the same symptoms, either of a milder or severer grade. How the cause was generated, I cannot tell. I *believe* it was atmospheric. Some one may say that this is not sufficiently definite. True, it is not as much so as we all would wish; but still it is as definite as our present limited knowledge of various other causes producing peculiar epidemics will justify. A field for speculation is here opened, but as this is not the place nor the occasion for such an enterprise, we will not enter upon it.

Upon the pathology of the disease we would also speak with much caution. The cerebral symptoms were very striking, peculiar, and uncommon. The spinal were not so much so, having several times seen similar symptoms in spasmodic complaints. In the ganglionic system there was evidence of great derangement; medicines had but little, and frequently no effect at all, so completely was that beautiful and important chain, which connects every organ, and holds them in friendly action, unlinked.

The form of cerebral disease in some patients was phrenitis; and in others (the greater number) it was arachnitis. The

symptoms immediately preceding death, were very much like those observed in children who die of acute arachnitis terminating rapidly in effusion. The fever was of a typhus form, and the *eruption*, so far as I have observed, is confined to this form of fever.* Bleeding was generally practiced after the manner given in the cases reported, and I cannot say that it always afforded even partial relief. Yet we could scarcely keep our lancets dry in the treatment of such cases.

Emetics at the onset always afforded relief, I thought, and I have reason to believe frequently arrested the disease at once. They had to be given, though, during the headache which preceded the chill. Purgation was of great advantage during the whole course of the disease.

The warm bath had a good effect in all such cases as the second one reported.

Dry cupping and friction, with stimulating articles along the spine, always afforded relief. Patients would frequently request those remedies to be reapplied, when it appeared to me that a *further* use of them would produce much pain.

Blisters to the nape of the neck afforded also considerable mitigation of the cerebral symptoms.

Of all the remedies, however, which I used in the treatment of the disease, I think I derived more benefit from opium than any other one article. It had a good effect, given in any form and almost at any time.

Note.—I have lately had one of the patients, who had this epidemic, under treatment for *deafness* induced by the disease. She could talk very well before she was attacked—did not have a very severe attack—recovered well, but lost her hearing. I have failed in restoring it. She has, in consequence of deafness, forgotten how to talk. She is four years old.

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* Since writing the above, I have seen a very interesting case of typhoid fever, in which the same eruption appeared very thick on the breast, back, neck and superior extremities.—*August*, 1842.

ART. III.—*A few practical Remarks upon Erysipelas of the Head.* By CHARLES S. TRIPLER, M. D., Surgeon U. S. Army.

For obvious reasons, inflammation has occupied a prominent place in the writings of medical authors, from the dawn of our science to the present day. The frequency of its occurrence upon the superficial tissues of the body, must of necessity have attracted to it a particular degree of attention. Its phenomena have, hence, been always as accurately observed and recorded as the state of physiological knowledge would admit. So palpable a distinction as that between phlegmon and erysipelas could hardly have escaped attention at any time; accordingly, we find it noticed among the earliest writers. But, from the imperfect knowledge, or rather the entire ignorance, of the ancients, of all just anatomical and physiological notions, we need not be surprised to find that they had no conception of the important varieties of each of these two kinds of inflammation.

To this remark, however, there is one exception, and that is, in the case of erysipelas affecting the head. The particular danger of this variety, was early noticed. Celsus is quite emphatic on the point—"Id autem," says he, "quod erysipelas vocari dixi, non solum vulnere supervenire, sed sine hoc quoque oriri consuevit, atque interdum *periculum majus* affert, utique si circa cervices aut caput constitit."*

But although he here distinctly recognizes the increased danger of erysipelas occurring spontaneously, and especially if affecting the head, he still seems to consider it as merely a variety of erysipelatous inflammation, and not as possessing any specific character. This distinction is comparatively of

* He goes on to prescribe blood-letting, refrigerants, discutients, &c. Among his local remedies he has anticipated the favorite application of Hennen and Liston in local inflammation—"Quicquid impositum est, beta folio contigendum est, et super, linteolum, frigida aqua madens, imponendum."

a modern date and even now is by no means universally, I might say not even generally admitted. I think it an important one, however, in a practical point of view, and I must confess my own opinion is decidedly in favor of its truth.

In the middle of January, 1840, I repaired to Detroit, Michigan, to assume the charge of the Army Hospital at that post. The garrison was then composed of a detachment of the 4th Regiment of Artillery, consisting of about 350 officers and men. Erysipelas of the head had appeared a short time before my arrival, and I found one man convalescent with it in the hospital, and one sick with the disease, who died soon after. Ten more cases occurred during the first quarter of that year. From that time up to last January, sporadic cases have from time to time presented themselves among our men, and I have frequently heard of other cases in the private practice of the medical gentlemen of the city.

Of the first ten cases, six came on with ordinary symptoms of fever, two with cynanche, one with parotitis, and one with pneumonia. The period of the developement of the eruption was from two to four days, and in one case (fatal one) fifteen days. The duration of the disease in the successfully treated cases was from eight to forty-four days. The cause of this affection, as it prevailed at Detroit, I am inclined to think, was the peculiar poison of typhus fever. An eminent authority observes, that "no very exact information has yet been established respecting the causes of erysipelas. We absolutely know nothing about the immediate cause; the prevailing ideas concerning the predisposing causes are vague; and only those causes termed exciting appear entitled to much confidence."

The causes usually enumerated are, external irritants, heat and cold, wounds and other mechanical injuries, intemperance, derangement of the digestive organs and contagion. Most of these causes can only be assigned as producing *accidental* erysipelas, the seat of which will be that part to which the cause was immediately applied. That form of the disease of which we are treating, must arise from a more specific agent. It was suggested to me that the cause of my cases, was the sudden al-

ternations of temperature to which soldiers are subjected when on guard in the winter season—leaving a close guard room, heated to an inordinate degree by stoves, standing post in very cold weather for two hours, and then returning to the hot room. This is not satisfactory; for most of the men of the command were subjected to the same influence in the course of the season, and yet comparatively few became subjects of the disease. And, moreover, one of the cases occurred in a man who had never been so situated; his neatness as a soldier causing him to be invariably selected as orderly to the commanding officer, when detailed for guard. I also frequently inspected the company quarters during the season, and never found the barrack room too much heated, nor imperfectly ventilated.* I suspected at one time, the cause might be found in the unfavorable construction and location of the main barrack—a building put up for a store house, built of wood with low lofts, three stories high, and situated immediately upon the margin of the river, on the lowest ground in the city. But, two companies of the detachment were quartered in another establishment, remote from the river, and on much higher ground. Among these two companies their due proportion of cases occurred. Again, some few cases occurred among the citizens, and one in a lady in the most extensive hotel in Detroit. One or more cases of the disease also occurred at the arsenal, twelve miles from Detroit.

Intemperance cannot be assigned as the cause, for two of the first ten cases occurred in men perfectly temperate, while among the many intemperate men of the command only eight more cases appeared. Intemperance can be considered only as a cause predisposing to disease generally, while the cause of the particular form the disease shall assume, must be sought elsewhere.

There is no field in the wide domain of medical enquiry,

* That is, the ventilation was as complete as it could be in such contracted quarters. There were too many men and too few windows for the size of the rooms.

so carelessly cultivated, as that of the etiology of diseases. A disease appears, and an attempt is immediately made rather to *assign* than to *discover* a cause for it. Something suggests itself which seems to satisfy the problem, while, in fact, it may have no relation to it whatever, and the most it should lay claim to, would be a low degree of probability. Nevertheless, it is frequently too readily adopted, and too generally assented to, until another writer shall (as is usual in our profession) first show the inadequacy of the cause assigned by his precursor, and then proceed to attribute the effect to another cause, in all probability equally conjectural. The truth may perhaps be hidden among the multiplicity of errors advanced, but who is able to find it?

Erysipelas of the head has thus been attributed to contagion, and strong facts are adduced in support of this opinion by Dr. Stevenson, Dr. Wells and others. This doctrine, however, can boast few supporters at the present day. It seems to be the fashion now to dispute the contagious nature of all febrile diseases. But the question can hardly be considered as settled. If it were even admitted that a disease were contagious, it by no means follows, in my opinion, that it can be reproduced in no other way. It is evident that the first subject of it could not have derived the poison from contagion. There must have been some peculiar conjunction of appropriate elements that resulted in the production of the poison in the first instance, and I can conceive of nothing that forbids the idea that such conjunction may again occur. Cholera, yellow fever, plague, typhus, &c., I cannot doubt are sometimes contagious, and sometimes of spontaneous origin. The contagionist shows conclusively, to my mind, that these diseases have been propagated by contagion, and if he would stop here, he would do well. But he must go a step further, and argue, that because they are sometimes contagious, they are always so, and can be reproduced in no other way; clearly a *non sequitur*, and he cannot so control facts as to bear him out. His antagonist is no better off; not content with demol-

ishing the position of the contagionist (which he can easily do) he must impale himself on the opposite horn of the dilemma.

After all, it would save a deal of useless labor and controversy, if observers would content themselves with a simple statement of facts as they occur, and when a sufficient number shall have been accumulated, perhaps an intelligible explanation of the origin of particular epidemics may be extracted from them.

But to return to the erysipelas at Detroit. I have expressed my opinion, that it was neither produced by intemperance, alternations of heat and cold, nor by the particular location of the barracks. I may now add that I saw no reason to suppose it contagious *in this instance.* What then was the cause? I do not know—I can only suggest, and that not without hesitation, that it was a form of typhus fever. The London Medico-Chirurgical Review, for July, 1839, contains an analysis of a work on typhus fever by Dr. Roupen. Among the forms which this disease assumed in the London hospitals, he enumerates erysipelas. "During the prevalence of the epidemic (typhus) in various years, idiopathic erysipelas has been a very common and a very serious addition to other complaints in persons exposed from their situation in hospitals and elsewhere to the vicinity and infection of typhus, and so common was it in the progress of the fever during the present year as well as during that of 1831, that no doubt could be entertained that it was essentially connected with and incident to this disorder." "It prevails at the same time as typhus, is preceded by the same symptoms and arises amongst nurses or those in attendance upon the patients ill with that fever."

It is a well ascertained and an admitted fact, that typhus, though undoubtedly "a disease of the whole system", still impinges more powerfully upon some one or more distant organs; and it would even appear, that in particular endemics or in particular districts, the disease seems almost invariably to seize upon the same organs to the entire exclusion of the rest. If this be true, it may account in a great degree for the

diversity of opinion among writers as to the seat of the disease—one placing it in the brain, another in the stomach, a third in the glands of Peyer and so on, while all are sometimes true, and none exclusively so. Now, why may not typhus sometimes expend its force upon the skin? What should give the poison that particular direction, I do not know; nor do I know why it should sometimes select the lungs or brain for its point of attack. The reason why some one or more of the glandular apparatus* is selected, I suppose to be the exalted vascularity of those tissues; and in this respect as well as others the dermoid tissue may be classed among them. If the pulmonary tissue were extended over a surface equal to that of the dermoid, I apprehend erysipelas would be as appropriate a term for the pneumonia of typhus as it is now for the cutaneous disease.†

Samuel Cooper remarks that, "Every surgeon is well aware that one cause of erysipelatous inflammation is a fever of a determinate and peculiar nature, one feature of which is, the invariable production of this kind of inflammation upon the surface of the body." If he had said, one cause is the poison of typhus under peculiar circumstances as yet unknown, he would have conveyed a practical, intelligible and just idea of this particular form of erysipelas.‡

The supposition, then, that the disease as it prevailed here, was a form of typhus, seems to me at least to be a plausible one. It occurred under the circumstances in which we ordinarily expect to find typhus; about the same proportion of persons exposed were attacked as in moderate typhus endemics, and the symptoms, exclusive of the eruption, were very similar to those of typhus. It should be stated, there were no cases of the usual form of typhus during the prevalence of the erysipelas.‖

* I have not used this term in its strict, but in a free sense.

† See Hunter on Inflammation.

‡ Erysipelas may also be one of the phenomena of an intermittent or any other form of fever.

‖ These remarks refer to the barracks only.

Dr. Good places erysipelas among his exanthemata. He has divided the general subject, with a view of drawing a line of demarkation between erythema and erysipelas proper. He considers the fever in erythema as only symptomatic, and the eruption the primary disease; while in erysipelas this order of cause and effect is inverted. Whatever may be the justness of his views with regard to erythema, I fully coincide with them in reference to erysipelas of the head. Mr. Lawrence disputes the propriety of placing erysipelas among the exanthemata, because it is often not preceded by fever,* because it is doubtful whether it is contagious, and because it attacks the same individual repeatedly. This is evidently taking a limited rather than an enlarged, a surgical rather than a philosophical view of the matter. No one pretends that every case of erysipelas is preceded by fever, but no one who has been favored with opportunities for observation, can doubt, that a particular form of fever may prevail, one of the phenomena of which will, in the generality of cases, be erysipelas. Now, if from effects we can sometimes infer causes, and if identity of both cause and effect be necessary to establish identity of *essence*, we may conclude that the disease under discussion is *not* the same either in cause or effect as the erythema of Good or most of the forms of the erysipelas of Lawrence; nor has sufficient yet been established by observation, to determine whether it ought, or ought not, to be classed among the exanthemata, even under the conditions prescribed by Mr. Lawrence. This is an important practical matter, and Mr. Lawrence seems to be in error in supposing, if I understand him right, the causes of erysipelas to be exclusively those of phlegmon, and the effect, merely a particular modification of cutaneous or cutaneous and cellular inflammation. Every case of erysipelas is not a case of typhus, nor is every pneumonia; but that both are sometimes symptoms or phenomena of typhus, I cannot doubt; and this is the mistake of Lawrence and others, in confounding the specific affection with the

* This will depend altogether on the *cause* of the disease.

erythema of Good or with erysipelas arising from extreme irritants, mechanical injuries, &c.

Burserius divides the disease into idiopathic, symptomatic and accidental. This division I should decidedly prefer to that of Mr. Lawrence, into erythema, simple, œdematous and phlegmonous erysipelas. I doubt whether an endemic erysipelas of the head ever prevailed, without exhibiting instances of all Mr. Lawrence's species, as symptoms of what I believe to be the true disease—typhus fever.

Mackintosh denies "altogether the idiopathathic nature of erysipelas, and believes it to be an occasional symptom of different diseases, which diseases may frequently occur under atmospheric, epidemical, and contagious influences." This general remark seems to cover the ground taken by Roupen and to sustain the opinion that it is sometimes a symptom of typhus. But I should judge from what follows in his (Mackintosh's) article on the subject, that he had excluded in his own mind the idea of typhus when he wrote the paragraph which I have quoted. He evidently considers it as an active inflammation, and requiring always the free use of the lancet. Now, I am far from disputing that such may have been the case in Edinburgh during his experience, but I am very confident that in other places its nature is sometimes very different, I cannot agree with him either, in his notion that the eruption ought to be considered a natural blister; at least in epidemic erysipelas. In my cases the constitutional disturbance did *not* subside as the eruption appeared.

Mackintosh goes on to quote Cullen's history of the symptoms of erysipelas of the head. That history is a very good history of the accession of inflammatory typhus. He then quotes Sydenham's letter on the plague of 1655, and from this it appears that that epidemic occasionally assumed the form of erysipelas. All these things serve, in my opinion, to corroborate the notion that the disease is a variety of typhus. As to Mr. Abernethy's saying that, "he would be hanged if erysipelas was not *always* the result of a disordered state of the digestive organs," it merely proves, that though a clever phy-

sician, that gentlemen was no prophet. Hippocrates and Galen before him, had assigned it a similar origin, and that it does sometimes arise from disorder of the digestive apparatus, there can be no doubt; but "*always*," is quite another matter.

I have dwelt more at length on this part of my subject, that I may be permitted to suggest to such of my professional brethren as shall take the trouble to read this paper, and who may be so situated as not to be likely to see cases of the disease in numbers (as in our hospitals), to be on their guard when they meet with a sporadic case, and not to decide too hastily that it is a mere inflammation. I assume it as a fact that sporadic cases of typhus are usually much more dangerous than cases occurring during an epidemic, and hence the first steps in the treatment are of great importance. I shall not trouble my readers with a general description of the disease, but shall content myself with quoting a single case, which will show the usual source and character of the disease as it presented itself to my observation at this post.

H——, a private of the 4th Regiment of Artillery, reported sick 14th March, 1840, with sore throat. He is intemperate and has been drinking lately. An emetic was prescribed and tinct. ammon. to be applied externally. 15th. Symptoms the same; sol. antimon. tart, ordered every two hours. 16th. Same; the inflammation is a diffused scarlet redness, with slight ulceration of the tonsils, but without any particular tumefaction; continued sol. ant.; sol. nit. argent. to the throat. Evening—blister to the throat. 17th. The throat is better, but during the last night erysipelas made its appearance on the cheek and nose, which spots had joined in the morning. Bowels open; tongue moist, with a slight yellowish fur; no headache; pulse natural; skin dry, and a little hotter than natural. Tr. iodin. to the face; cont. nit. argent. to the throat; cal. gr. iij, p. Doveri gr. v. every three hours. Evening—more excitement; tongue dry; inflammation and tumefaction extending to the other side of the nose. Continued cal. and p. Dov.; applied sol. mur. hyd. to face. 18th. Inflammation extends to the whole of the right side of the face, including

the parotid, eye, and eyelids, and slightly affects the left side. The sol. mur. hyd. has already cauterised the oldest inflamed spots. The pulse is full, soft, equable, but somewhat quickened; tongue moister than last night, less furred, but shrivelled; bowels open. Continued remedies. Evening—swelling extends to the scalp and left cheek; pulse soft, full, and rather frequent; skin moist; bowels not moved to-day; mind clear; continued remedies. 19th. Tumefaction same as last night; slept well; bowels not moved; pulse 100; skin soft, but hotter and dryer than natural; tongue dry, wrinkled, with brownish mucus on the sides. Enema; continue cal., p. Dov. and sol. mur. hyd. to the face. 20th. Slight febrile heat; tongue dry and brownish; thirst increased; bowels open; stools black; pulse soft, 104; no delirium; no local pain; slept well, parotid rather more swollen; cuticle begins to shrivel and detach itself; ordered sol. bicarb. potas. 21st. Five dejections since last evening, lighter in color and thin; slept well when not disturbed by the bowels; no delirium; tumefaction subsiding; tongue dry and chapped, but not glazed; pulse 92, soft; and of sufficient volume; skin soft and dry; temperature slightly elevated; yeast poultice to face; continued sol. potas. 22d. Rested well; skin natural; pulse 88; tongue becoming softer at the edges, still shrivelled and wasted in the middle; bowels open; stools becoming lighter and more consistent; cuticle detached over the whole inflamed surface; pus about the eyes, as in small-pox; inflammation of the eyes does not affect the cornea. 23d. Skin dry; temperature natural; pulse weak, 80; tongue dry, brown, and shrivelled in the centre, moist and red at the edges; some slight indications of typhomania exhibited in a desire to get up and sit by the window; bowels twice moved in the night; stools thin and yellow; infus. serpentariæ every three hours. 24th. Skin natural; pulse 84; tongue moister and cleaning off; inflammation of the eyes has subsided; desquamation of the cuticle going on; bowels open; stools light colored and of tolerable consistence; slept well last night; continue serpentaria. 25th. Pulse 72; skin cool; tongue soft and moist; slight mercurial fetor of the breath;

bowels open; desquamation of the cuticle progressing; tumefaction gone, except on the anterior portion of the right parotid, which threatens an abscess; continue serpentaria. 26th. Skin natural; pulse 68; tongue moist and almost clean; slept well; two stools since last night, improving in character; slight degree of deafness; inclined to doze. 27th. Still improving; parotid proceeding to suppuration. Patient out of danger.

The case above cited, I have presented as exhibiting the phenomena of the disease as it occurred in Detroit, in its simplest form. I quote it as a sample of the features of the inflammation, rather than with a view to determine from it the nature of the fever. Many cases were much more severe as to the constitutional symptoms; the local disease seldom or never extended farther. In the severer cases, there was more delirium, more prostration; the stage of collapse was more threatening, demanding the exhibition of more powerful stimulants. There was no sloughing of the cellular tissue, and I believe *that* is not an ordinary occurrence in this form of erysipelas. After the subsidence of the specific inflammation, small local abscesses occasionally occurred on different parts of the head, sometimes on the scalp, in one case in the cellular tissue of the eyelids, and in the case cited, in the parotid region. Swelling of the parotid was a common occurrence. In a large proportion of the cases, deafness supervened, as the erysipelas subsided. Both deafness and local abscess about the head, have frequently occurred in cases of typhoid fever that have fallen under my observation.

Upon a review of the phenomena of this disease, and the circumstances under which it occurred, both during the period which more particularly suggested these observations and since that period, I think it ought to be considered as a form of typhus, and that its appearance should be attributed to the presence of the poison, whatever it may be, that produces that disease. From the opinion I have expressed as to the *nature* of the disease, it will readily be inferred that I consider a proper *constitutional* treatment of paramount importance. And herein do I differ most widely from those who treat of

erysipelas as a modification merely of inflammation. But without occupying time or space in recapitulating the treatment of others, I shall proceed to give my own in as few words as possible, merely premising, that whether right or wrong in my notions of the pathology of the disease, my treatment has been attended with gratifying success.

Blood-letting, at the onset, was of singular service. Though I never saw it arrest the disease at once, as Mackintosh says it frequently does, still it has evidently rendered the inflammation milder, and above all, has checked the general morbid action. Whether it would be as beneficial in future epidemics, is a matter of uncertainty. There can be no doubt that the epidemic meteoration of different seasons decides the character of those seasons, so that it requires renewed observation in each season to determine the applicability of this as well as other heroic remedies. The general character of the diseases which prevailed at Detroit during the early part of 1840, was inflammatory. Blood-letting was always borne well, not only in my practice, but in that of the medical gentlemen in the city. Even in the habitually intemperate, it answered well when they were subjects of erysipelas.

An early exhibition of emetics and purgatives, or both, was always resorted to, on the accession of the disease. After these things had been premised, the permanent constitutional and local treatment was at once adopted and persevered in. The former consisted of calomel and p. Doveri, in small and frequently repeated doses, during the stage of excitement, with infusion of serpentaria, wine whey, carb. ammonia, &c., during the stage of collapse; the latter, of a strong solution of the deuto-chloride of mercury.

It is not necessary to say any thing more of the *general* means, but the *local remedy* has proven so eminently successful, that I cannot omit to recommend it strongly to the notice of the profession.

I had made trial of the usual evaporating lotions, the mercurial ointment, the sol. nit. argent., &c., without any satisfactory result. The nitrate of silver I consider the best among

them, but it does not produce the immediate and powerful impression that follows the use of the corrosive sublimate. This remedy was suggested to me by Dr. Pitcher, of Detroit (formerly of the army,) who had received it from his preceptor. I have never seen it recommended in books. Dr. Pitcher told me, that his preceptor thought it not only arrested the local disease, but also protected the brain. Of the latter I have no doubt, though I think it does not prevent the inflammation from extending pretty generally over the face and head. If any of my readers should be induced to try the remedy, I warn them not to expect the spread of the inflammation to be forthwith arrested by it; if they do, they will be disappointed. I was in the habit of applying it always a little beyond the bounds of the inflammation. It produced no effect upon the sound skin, but on the diseased integument, its effect was rapid and decisive, twenty-four hours being sufficient to cauterize the cuticle completely. It must have acted as a powerful counter-irritant in relation to the brain, for after its application, notwithstanding the apparent severity of the local disease, the brain rarely, and even then but slightly, suffered. When delirium occurred it was always moderate and never much protracted. It was remarkable, that during that winter all the cases treated with the corrosive sublimate, as far as I know, recovered; while two, to my knowledge, treated without it, died. Since then I have witnessed two unsuccessful cases; but the entire ruin of the constitution in those cases from intemperance and all sorts of vice, was sufficient to account for the disastrous result. I therefore look upon this remedy as an important means in the treatment of erysipelas of the head. The solution I used was composed of 20 grains of the deutochloride of mercury to one fluid ounce of water. A friend of mine in the city used a saturated solution in a frightful case, successfully. It is applied with a pencil or piece of lint over the inflamed surface and a little beyond, four or five times in the course of a day, taking care to guard the eyes, nostrils and mouth. During the prevalence of the complaint, I met with some observations by Mr. John Davies of Hertford, England

on the use of iodine in various local diseases. Among others, he recommends it strongly as a topical application in erysipelas. I made trial of it according to his plan in two cases. One was very mild, with the slightest degree of constitutional disturbance, and the local involving merely one side of the nose and a small portion of the cheek. In this case it was successful. The other case I have recorded above. It did not seem, in this instance, to produce the least effect during the short time I used it, and as the symptoms were alarming, I did not feel justified in relying upon an untried remedy, while I possessed one that experience had shown to be worthy of dependance. I therefore fell back upon the corrosive sublimate. I do not profess to have given the tr. iodine a decisive trial, but I must confess, from my experiment, I should not place much reliance upon it, and I consider it far inferior to the other remedy.

Detroit Barracks, Nov. 1842.

Bibliographical Notices.

Art. IV.—*Elements of Surgery.* By Robert Liston, Surgeon to the North-London Hospital, Professor of Clinical Surgery, &c., &c. From the second London edition, *with copious notes and additions,* by Samuel D. Gross, M. D., Professor of Surgery in the Louisville Medical Institute, Surgeon to the Louisville Marine Hospital. Illustrated with numerous engravings. Philadelphia: Ed. Barrington and Geo. D Haswell; Louisville, James Maxwell, Jr. 1842. pp. 640, 8vo.

We do not know any work that has been received with such general commendation as this of Mr. Liston. The first edition rendered the author's name familiar to the profession in this country as that of a most dextrous and skilful surgeon, and an acute and original thinker. On its first appearance, it took rank immediately as one of the most valuable treatises in the language, and every year has but served to render it still more popular. It has proved equally acceptable to the juniors and the elders of the profession. To the former it particularly commends itself by the general perspicuity and vivacity of style, the plain, straightforward mode in which every thing is discussed, the simplicity of the rules of treatment and the clearness with which they are laid down; to the latter, by the useful hints it contains, and the boldness and originality of many of the author's opinions.

To give an analysis of a work so well known, would be useless, even if our space warranted it. It is sufficient to say that the present edition, having been carefully revised by the distinguished author prior to its publication abroad, is improved or

changed so far as it may be supposed the experience of several years spent in the constant practice and cultivation of surgery, would extend his knowledge or modify his views. Nevertheless, as it came from the English press it was still imperfect, and the labors of the American editor are intended to supply the deficiencies and omissions apparent in the English edition.

The notes and additions of Dr. Gross amount to about seventy-five pages in all. Among the latter we notice articles on strabismus, club-foot, hydrocele or encysted tumor of the neck, hydatic tumor of the bone, &c. The notes are numerous, dispersed throughout the volume, and will be found to embody a mass of valuable information. We cannot forbear making a few quotations from this portion of the work, short as is the space allotted to us.

In the article on club-foot, Dr. Gross is disposed to adopt the views of Guerin of Paris, who "supposes that the primary mischief is in the nervous system, and that the spasmodic and permanent shortening of the muscles of the affected limb, is altogether consecutive." He deprecates the indiscriminate use of the knife for the cure of this deformity, and thinks that a majority of cases occurring in young subjects may be relieved without a resort to cutting.

"It is still a disputed point, whether, in the treatment of this affection, particularly in infants and young subjects, it is necessary or even justifiable, to divide, as a preliminary step, the tendons of the muscles which are instrumental in keeping up the distortion. Without endeavoring to settle this question, for which the time has not perhaps yet arrived, I must express my conviction that the present rage for tenotomy is calculated to do a vast deal of harm, not only in individual cases, many of which do not require it, but, what is worse and more deeply to be lamented, in bringing discredit upon an operation, which, if judiciously performed, cannot fail to be of the greatest benefit. In most of the cases occurring in children under two or three years of age, division of the tendons is altogether unnecessary; indeed, one of our most distinguished orthopedic surgeons, Dr. Chase of Philadelphia, seems to trust almost entirely to the employment of apparatus, and to resort to tenotomy only in the worst grades of the disease."

In the treatment of hydrocele, of the several varieties of which the text gives a very good account, Mr. L. prefers the method by injection, using for this purpose the pure port wine. The editor, on the contrary, objects to this method, as being not always successful, and sometimes requiring frequent repetition before a sufficient amount of adhesive action is obtained to obliterate the vaginal sac.

"Moreover, by carelessness on the part of the surgeon, the canula may slip out of the vaginal sac, and so allow the fluid to pass into the cellular substance of the scrotum, where, if it be not speedily evacuated by free incisions, it is sure to occasion gangrene. But this is not all. The operation, even when well performed, is sometimes followed by violent inflammation and suppuration; in one instance, indeed, I knew it to be productive of tetanus."

These considerations, he goes on to say, should be sufficient to deter the practitioner from resorting to this remedy, especially when it is remembered that we have another, not only entirely devoid of danger but always successful.

"This remedy is the seton, which I have been in the habit of employing, in repeated instances, for some years past, and from which I have never experienced any other than the most happy results. The operation is perfectly simple, the amount of inflammation produced by the presence of the foreign body may be easily regulated, and there is no danger of sloughing of the scrotum, much less of the developement of tetanus, or other mischief."

Certainly, so simple a proceeding is far better than the paraphernalia of elastic bottles and brass stop-cocks. The same means is recommended by Dr. Gross in the radical treatment of what has been denominated hydrocele of the neck, an encysted tumor occurring in that region, and met with in both sexes and at various periods of life.

One of the most interesting and valuable notes appended to the work, is that on wounds of the intestines, accompanying penetrating wounds of the abdomen. The directions given for their management by Dr. Gross, possess peculiar claims to our confidence from the fact that they are founded on the results of original investigations pursued by him during the past

summer. These results entire, constitute a highly valuable paper, which we shall have the pleasure of presenting to our readers in the next volume of the journal. According to Dr. Gross, the grand principle which should guide the surgeon in these cases, is to close the opening so completely as to guard against the effusion of fecal matter. This done, the patient is comparatively safe, or free from the danger of peritoneal inflammation.

"To attain this object, the continued, or glover's suture as it is termed, is unquestionably preferable to any other, especially when made, as I would suggest it should be, with a small sewing-needle, armed with fine silk, and passed between the muscular and mucous coats, or, what is the same thing, through the substance of the cellulo-fibrous lamella. After the suture has been applied, the protruded part of the mucous lining, if there be any, should be pared off with a sharp knife, to facilitate the process of reparation, the surface of the bowel should be cleansed with tepid water, and the whole carefully returned into the abdomen. If the interrupted suture be used, the interval between each two respective threads must not exceed two lines, or the sixth of an inch, otherwise there will be danger of fecal extravasation, and the ends, instead of being brought out at the external aperture, should be cut off close to the knots. The reason why I prefer the continued suture, made in the manner above mentioned, is simply because we can thereby more effectually close the wound, at the same time that the parts are placed in the best possible condition for speedy reunion, from the want of protrusion of the lining membrane, and consequently the more perfect contact of the serous surfaces.

The ligatures which are employed in sewing up a wounded intestine are detached at a period varying from ten days to three or four weeks, according to the nature of the suture. When the extremities are cut off close to the knots, they invariably fall into the cavity of the bowel, and are finally discharged along with the feces; if, on the other hand, they are brought out at the external opening, they pass off in that direction instead of the one just mentioned."

If the opening in the gut does not exceed three or four lines in extent, Dr. G. recommends the plan of Cooper, to encircle it with a ligature. The latter should be drawn pretty firmly

to prevent slipping, and the ends cut off close to the knot. It generally makes its way into the bowel in eight or ten days.

"When the bowel is completely severed, or mortified in its entire calibre, the edges, after being properly prepared, should be brought in contact, and retained by the continued or the interrupted suture. Cases of this kind, although apparently desperate, are not always of so hopeless a character as might at first sight be supposed. This is shown, not only by experiments on the inferior animals, but by what occurs in the human subject, in sphacelated hernia, and in intus-susception. In the former, the greater part, or even the whole, of the circumference of the tube may be destroyed, and yet the patient ultimately recover, with perhaps the temporary inconvenience merely of an artificial anus; and in the latter, large pieces are not unfrequently detached without any serious suffering, save what is experienced during the antecedent and concomitant inflammation. In my morbid collection is a preparation of this kind, evidently a portion of the colon, nearly a foot long, which was discharged by a child six years old, who, notwithstanding, made a speedy and perfect recovery. Thirty-five cases of a similar character, collected from the writings of different pathologists, have been reported by Dr. Thompson of Europe. In a dog, from which I removed two inches and a half of the ileum, and treated the edges of the wound with six interrupted sutures, complete recovery took place, unattended with a single bad symptom. The threads were introduced at equal distances from each other, with a small sewing-needle, and the ends cut off close to the knots. Four months after the operation, being in good health, and the outer wound entirely healed, he was killed. Externally the bowel was perfectly smooth and natural, as if no injury had ever been inflicted upon it: the mucous membrane was of the same appearance as elsewhere, with the exception of a small depression corresponding with the edges of the wound."

These quotations, which we have taken at random, without any reference even to the order in which they occur, will perhaps suffice to give the reader an idea of the repast that awaits him in the book itself.

We must not forget to mention that the volume is rendered still more attractive by the addition of numerous wood engravings (some of them introduced by Dr. Gross), all finely executed. These will be found of very considerable advantage to

the student, materially assisting him in comprehending the explanation of morbid structure. Another admirable feature, is the printing of the notes in type of the same size as that of the text. This obviates almost entirely, whatever objections can be alleged against foot-notes. C.

Art. V.—*The History, Diagnosis and Treatment of Typhoid and of Typhus Fever; with an Essay on the Diagnois of of Bilious Remittent and of Yellow Fever.* By Elisha Bartlett, M. D., Professor of the Theory and Practice of Medicine in Transylvania University. Philadelphia: Lea & Blanchard. 1842. pp. 393, 8vo.

This is a neat, unpretending volume, aiming, as the author expresses it, "at no other merit than that of being a methodical and compendious summary of the actual state of our knowledge upon two of the most common and most important diseases." Such a work supplies a want extensively felt by the the profession in this country. As justly remarked by Dr. Bartlett, many authorities on the subject of idiopathic fevers that were received as standard and classical, only a few years ago, are fast becoming obsolete. The treatises of Fordyce, Wilson Philip, Armstrong, Southwood Smith, and Tweedie, describe a form of fever rarely encountered by the American practitioner; and they are also very deficient in their pathology, as most of them are in their plans of treatment, which, however successful they may be in the disease as it appears in England, are not well adapted to the fevers of the United States. These works, in a word, are very far from representing the actual state of our knowledge upon the subject of fever. Other treatises on fever are either not in general circulation, or else are not well suited to the profession here, on account of the minuteness and complexity of their details.

Dr. Bartlett, without aiming at any thing original, has presented, in his treatise, a history and comparison of the two

chief forms of fever, as they are now ascertained to exist, fuller and more discriminating than had been written. He has brought out clearly and strongly the means of diagnosis between the different species of fever; and in regard to the treatment of continued fevers, he has given a faithful account of the methods employed by the most eminent practitioners who have written on the subject. The full table of contents will enable the reader to consult any portion of the work without trouble, and on every head the details will be found accurate and clear, and, perhaps, minute enough for the student, and for the practitioner of medicine.

In his reference to the controversy concerning the contagious qualities of yellow fever, Dr. Bartlett has fallen into the common error of ranking Dr. Rush among the earliest advocates of the doctrine of the non-contagiousness of this disease. One of the most zealous and successful advocates of the doctrine of its domestic origin unquestionably he was; but he was a decided and unwavering contagionist until the year 1806 or 7, as his pupils of that period testify. About that time he surrendered the opinion that the fever was contagious, and announced the change of his views in the New York Medical Repository. These facts ought to be known, and it is due to historical accuracy that the position of Dr. Rush should be distinctly stated, as it is but just to Dr. Caldwell to admit, that he preceded Dr. Rush many years in avowing and defending the non-contagious nature of yellow fever. Dr. Caldwell, during a warm and protracted controversy on the subject, is believed to have been the only *public* and *writing* advocate of the doctrine in this country. We have his authority for saying, that he was supported in his opinion by the concurring belief of Dr. Physick, but this gentleman, as is well known, wrote but little on any subject, and never participated publicly in this dispute. Y.

Art. VI.—*Catalogue and Circular of the Medical Department of Laporte University.* First Session, 1842. Laporte, T. A. Stewart.

We were not a little surprised to find on our table, some weeks ago, a small document bearing the above title. Of a truth, we would as soon have thought of looking for Pompey's pillar as for a medical school at Laporte, Indiana, twelve miles from Lake Michigan. Ten years ago, the place where the town now stands, and the whole region round-about, was an immense wilderness, whose silence was unbroken save by the screech of an owl, or the yell of a Pottawatomie. Within these short ten years—nay, in a much shorter period—the country has been reclaimed from a state of nature: lands have been surveyed, and marked off into counties; towns have been built; farms improved—every thing, in short, has been added and done, that a dense and busy population, collected silently but swiftly from all quarters of the Union, could add or could do. And here last of all comes a medical school, which, like our Louisville Medical Institute, is designed to be the nucleus of a large and flourishing University.

Many perhaps who read this, and who, like ourselves, have been negligent of the rapid progress of improvement in the whole north-west, will smile as we did on first seeing this announcement. But let none such be deceived. If they will take a map of the United States, they will perceive that Laporte University stands in the centre of an immense region of country: Northern Indiana, the northern part of Ohio and Illinois, the whole of Michigan, Iowa, Wisconsin—a vast region indeed, already occupied by a large population, and capable of sustaining a much larger one, which must be supplied with physicians and surgeons. The enterprise, then, evinces more of judgment and prevision, than might at first be supposed. Moreover, the spirit and energy that have thus far characterized the founders of this university, are not merely deserving of all praise, but give favorable augury for the future. The charter was

procured about the middle of last winter; operations were immediately commenced, and *courses of lectures delivered to a medical class of fifteen pupils, and a law class of ten;* and since that time, as we are informed, a commodious Medical Hall has been erected, which is doubtless now occupied.

The Medical Department is organized as follows:

Daniel Meeker, M. D., *Professor of Anatomy, Physiology and Surgery, and Dean of the Faculty;* G. A. Rose, M. D., *Professor of the Theory and Practice of Medicine:* J. P. Andrews, M. D., *Professor of Obstetrics and the Diseases of Women and Children;* Franklin Hunt, M. D., *Professor of Materia Medica, Botany and Medical Jurisprudence;* J. B. Niles, A. M., *Professor of Chemistry and Natural Philosophy;* J. G. Newhouse, M. D., *Demonstrator of Anatomy.* The requirements for graduation are similar to those of the best regulated schools in the country. The candidate must be twenty-one years of age, of good moral character, have studied medicine three years, including two full courses of lectures, one of which must have been in this institution. Three years' practice and one course of lectures will entitle the student to an examination for a degree.

Attached to the circular are some complimentary resolutions passed by the class at the close of the session. We copy the last in the series, as being at once novel and characteristic:

"Resolved—that we now adjourn to meet again in this place on the second Monday of next November."

And we have no doubt that every mother's son of them have done it! Your Hoosier is not easily baffled; we speak from a twenty years' knowledge of the people. C.

Disease of the Kidney.—Dr. Barlow has narrated a series of interesting cases illustrative of the following points:—First, that there is a certain symptom connected with irritation of the kidney, and which, although not confined to it alone, is not necessarily connected with disease of those structures whose affections are most likely to be confounded with those of the kidneys, so that when this symptom is absent, we may eliminate, and thereby disembarrass ourselves of the consideration of all such affections of the kidneys as would necessarily be attended by irritation of that organ; and when this symptom is present, we are furnished with a reason for assigning the seat of the disease to the kidney, in preference to several adjacent structures. The symptom is sickness, or irritability of stomach.

Secondly, and further, there is a certain symptom, or rather, set of symptoms, dependent on the non-depuration of the blood by the kidney, whether this non-depuration be the result of mechanical obstruction to the flow of the urine, of a diminution in its quantity, or of a depraved condition of that secretion, in which its most important ingredients are wanting—viz., cerebral disorder of a peculiar character.

There are, however, several affections of the kidney which do not necessarily give rise to irritation of that organ, and in which the sickness *may* not be present to aid our diagnosis; of these, the principal are the granular degeneration, suppuration, and perhaps some forms of adventitious or malignant deposit.

In the first of these, the disease, depending in the first place probably upon congestion, and afterwards upon a chronic change in the structure of the organ, irritation is not a necessary concomitant, although the disease may, and no doubt frequently does, result from irritation, giving rise to chronic inflammation; accordingly, we find that sickness is not always present, although it is sometimes a most troublesome symptom. In suppuration we should generally have irritation, giving rise to sickness, in the first instance, and probably, at a later pe-

riod, obstructed function, with its concomitant changes in the urine, and cerebral disorder, with probably, at some time, the presence of pus in the urine.

With regard to malignant disease, it is very probable that this deposit taking place, as has been remarked by Dr. Bright, in the cellular membrane connecting the firm parts of various structures, the parts of the organ in which this deposit takes place may be, as it were, pushed aside, without suffering any mechanical violence or functional disturbance; at the same time, it is also probable that, in the progress of distension to which this organ is subjected, some slight laceration, or irregular pressure, giving rise to irritation, will sooner or later occur, and accordingly some sudden invasion of sickness almost uniformly happens before such deposit in the kidney has produced a tumor of any considerable size. We should also in this case, probably, have some degree of hæmaturia, especially if the disease be of a fungoid character, the most common form of malignant deposit in the kidney.

Irritability of the stomach is not to be regarded as pathognomonic of disease of the kidney: but in cases where doubts arise as to whether any disease is to be referred to the kidney, or some neighboring organ, the absence or presence of sickness will go far to decide the question; and further, in cases not unfrequently met with, where the prominent symptom is distressing and obstinate sickness without any assignable cause, especial attention should be directed to the kidneys, and every means used to determine the state of those organs. The same observations will apply very nearly to the second class of symptoms—namely, the cerebral disorder; for as yet we want sufficient evidence to prove that similar derangement of the function of the brain may not be produced by other causes. The cases narrated are nine in number.—*Prov. Med. Journ.*, July 2, 1842.

On the Blood-Globules, their Formation and their Use.—M. Donne recognizes three kinds of particles in the blood,—red globules, or blood-globules, properly so called, white globules, and globulines of chyle, as he calls them. The red globules are flat in all kinds of blood; circular in the mammalia; elliptical in fishes, reptiles, and birds. The elliptical globules alone have a solid substance in their interior: no similar appearance has he been able to detect in the circular globules. Water, he says, renders the flat blood-globules spherical; and acetic acid

dissolves completely the round globules of the mammalia, but leaves an insoluble portion in the elliptical globules of birds, reptiles, and fishes. This insoluble portion, he says, is the central nucleus. He therefore regards the blood-globule as composed of a flattened vesicle containing a solid nucleus in the elliptical globules, and a fluid only in the circular ones. The elliptical blood-globule of the camel he found no exception to the general rule that those of the mammalia have no central nucleus.

The white globules he describes as colorless, irregular in their contour, and having a granular aspect, and exist in the blood of all animals. Water breaks them up, and he imagines them to be composed of a cyst containing three or four solid granules.

The globulines again he describes as extremely small granules similar to those of the chyle.

M. Donné gives the following theory of the mode of formation and uses of these different parts. The globulines are the product of the chyle, which is continually being added to the blood. Three or four of these unite together, and whilst circulating with the blood, receive an albuminous envelope. They thus form the white globules. The white globules once formed change little by little their forms; become flattened, colored; the granular matter in their interior becomes homogeneous or dissolved, and they are thus transformed into the red or proper blood-globules. The blood-globules themselves have only a passing existence; they dissolve after a certain time, and constitute the so-called *liquor sanguinis*. He says that certain substances, as milk, are capable of being immediately transformed into blood-globules by being ejected into the blood-vessels; and he regards the spleen as the organ more especially charged with the important function of the manufacture of blood-globules. He adds, that a minute examination of the vascular tissues shows, that at no point do the blood-globules leave their vessels to assist in the formation of the organs of the body, or to unite with other organic elements. Hence, he infers, it must be the fluid part of the blood which transudes through the vascular walls, and that this part must be the fluid essentially concerned in the process of organization.—*Edin. Med. and Surg. Jour.*, July 1842, from *Comptes Rendus*, March 1842.

On the Coloration of the Bones by Madder.—MM. Serres and Doyere have been engaged for upwards of two years in a long course of experiments on the coloration of the bones of living animals by feeding them with madder, and have at length arrived at some very curious and interesting results both with regard to that subject and the mode in which bone is formed. They found that when an animal was fed for a long time on madder, not only were the bones colored of a rose hue, but all the other solids and fluids of the body, with the exception of the tendons and cartilages and the white substance of the brain. In the bones alone they found this color to be permanently fixed, and not removable by maceration, nor by certain chemical solvents of the coloring matter of madder. Maceration, however, removed the coloring matter from all the other tissues. They ascertained, from a chemical investigation, that it was the phosphate of lime which held permanent the coloring matter of madder in the bones. They found that the coloring matter did not penetrate the bones to any depth, but the distribution of the color was remarkable as throwing much light on the true structure of the osseous tissue.

When a long bone colored with madder, is sawed transversely, the colored portion is observed to penetrate but a little depth, forming, as it were, a circle round the bone. When this section is examined by means of a magnifying power of twenty diameters or so, the coloring matter is seen to exist as red points scattered over a white ground. When a power of two or three hundred diameters is used, each point is seen to be in reality a colored circle surrounding a round aperture in the substance of the bone. A longitudinal section of the same bone exhibits the true structure of the bone more particularly; and proves that this rounded aperture seen surrounded by a colored ring in the transverse section, was the section of a minute canal, or capillary vessel, which is more delicate as the animal is older. The colored ring surrounding it was seen to be the walls of this canal, or that part of the bony structure which constituted its walls, and which differed in no respect from the uncolored portion of the bony tissue which surrounded it.

The most exterior layer of coloring matter seen on the outer surface of the bone was found to consist of a colored layer, as thin as that which was seen surrounding each bony capillary vessel in the interior of the bone. The bone then was seen only to be colored at those portions which were in imme-

diate contact with the capillary vessels of its tissues or of the periosteum, and at no other part.

MM. Serres and Doyere next pass to the examination of what it has been the custom of late to describe as the osseous corpuscles, and they show, by the simplest of all experiments, viz. by allowing a drop of oil to come in contact with a slice of bone under the microscope, that these so-called osseous corpuscles, on which so much stress has been laid, are neither more nor less than microscopic deceptions,—are, in fact, cavities or empty cells, which they think are possibly filled with a fluid during life, but not with the circulating fluid, as they they are never colored with madder.

These experimentalists ascertained another important fact. They fed a young pigeon with madder from the 10th of March to the 15th of April, 1840, when they amputated the wing, and found the bones deeply stained with the coloring matter. The madder was then stopped; it was nourished on the ordinary food of these animals; and on the 30th of January, 1841, the other wing was cut off, from the effects of which operation it died. Though this was a young animal, only four months old when it lost its first wing, and though the bones had been exposed to the incessant vital actions for so many months after the madder was stopped, its bones, when it died, presented the very same depth of color as the wing which had been amputated so many months before. MM. Serres and Doyere, therefore, justly infer, that the asserted incessant change or renewal of the molecules is not an essential condition of the living tissues, unless bones are to be ranged among the dead ones.

From these investigations, then, it appears that the staining in the bones by madder is purely a chemical process,—a species of dyeing. That in the colored ring which the naked eye discovers on the long bones, the coloring matter is confined to that portion of the osseous tissue in contact with the periosteum, and that forming the immediate walls of the capillary vessels of the bone. That, as the distance of each capillary vessel from each other is more than double the breadth of the colored layer surrounding each, even in the colored ring the greater portion of the bone is really uncolored, really white. And, lastly, that the dyeing of the bone is influenced by the mode in which the blood is distributed through it; some bones, from the peculiarities of their circulation, being colored from within outwards, otherwise from without inwards.—*Edin. Med. and Surg. Jour.* July 1842, from *Comptes Rendus*, February, 1842.

Physiological Observations on Double Uterus.—M. Dumas relates in the *Journal de la Soc. de la Méd. Pratique de Montpelier*, a case of double uterus observed by himself, and refers to a number of similar cases, from a comparison of which he draws the following conclusions.

1. The menstrual discharge may continue from the empty uterus, whilst the other contains an embryo.

2. The two uteri do not influence each other to such a degree, as that the empty one should be always under the immediate influence of the other which is fecundated.

3. The woman may be a virgin as regards the left one, and with child in the right, and *vice versa*. She may be at the full time with the one, and in labor, while with the other she is not near her period.

4. Superfœtation may take place in cases of double uterus with double neck and os uteri, and also in cases of double uterus with single neck and os uteri.

5. Notwithstanding the absence of anatomical proof of the existence of superfœtation in cases of double uterus, its possibility ought to be admitted in legal medicine.

6. In well marked cases of double uterus, there will be an inclination to the same side that the fertile womb is on; and all the symptoms will be slightly different from those observed in a case of single uterus.

7. Although it cannot be laid down as a principle, that each cavity of the double uterus can become so developed, as to be able to contain a fœtus at the full period; yet we can believe in frequent abortions.

8. The double uterus, when enlarged, renders labor and the expulsion of the after-birth difficult, and facilitates the laceration of the soft parts.

9. Thanks to the form observed by M. Martin St. Ange, (in which the cavity of the neck of the uterus communicated with that of the body, by a narrow canal thirteen millimetres in length,) we can explain the retention of the secundines, and the difficulty experienced in their extraction, when the canal which retains them has not been sufficiently dilated.—*Amer. Jour. Med. Sci.*, from *Gazette Méd. de Paris*, 4 Feb. 1842.

Mode of Preserving Nitrate of Silver.—M. Dumeril has for a long while employed a very simple process for preserving the nitrate of silver from the injurious effects of exposure to

the air, when run into sticks. It consists in merely coating the caustic with engraver's sealing-wax, which contains a large quantity of shellac. This wax adheres very well, and forms a strong and smooth varnish, as it were, which remains unaffected by the atmosphere. Thus protected the nitrate no longer stains the fingers, injures the caustic-case, nor is in any way changed by the moisture in the air, possesses a greater degree of solidity, and, at the same time, the process is of exceeding service in practice, inasmuch as when wanted for use a small part only of the caustic need be uncovered by means of a penknife, so that its application can be restricted to the part where it is required. This is of peculiar utility in ulceration of the throat, aphthæ, fissures, &c.—*Prov. Med. Journ.* July 2, from *Bull. de Thérap.*

External Application of Croton Oil.—Whenever it is required to use this method of counter-irritation, M. Bouchardat strongly recommends a plaster which has been much used by M. Chomel at the Hôtel Dieu, and which is thus prepared: Four parts of diachylon-plaster are melted at a very gentle heat, and while it is half liquid one part of croton oil is mixed with it, and the mixture is then spread in a thick layer on calico. Pieces cut from this may be applied to the skin, like ordinary sticking-plaster, and quickly produce an active irritation.—*B. & F. Med. Rev.* July 1842, from *Bull. Gén. de Thérapeutique*, March 1842.

Nature and Treatment of Scrofula.—After enumerating the different forms of scrofulous cachexia, Dr. Roesch arrives at the conclusion that scrofulous affections are produced by an excess of acid matters in the fluids of the body. Agreeing with the ancient physicians in his theory of the disease, he recommends their plan, viz. absorbents, alkalies, and fat or oily matters. He says he has observed that, in those countries where the children get a quantity of lard and other fat matters with their food, scrofula is extremely rare. Cod-liver oil, is therefore, according to him, one of the most suitable remedies to administer in this disease, seeing it possesses the rare properties of being at once a stimulant, a roborant, an antacid and a nutrient. He considers that the iodine in it will have a very secondary effect, the other properties of the oil being considered the most valuable.—*Ed. Med. and Surg. Journ.*, July, 1842, from *Haeser's Archiv.*, Oct. 1841.

State of the Blood in Hydrophobia.—The last number of the Austrian *Yahrbücher* contains an interesting case of hydrophobia, with an account of some experiments performed by Professor Berres on the patient's blood.

The blood was dark red in color, and of an oily feel; with the exception of the portion examined in the heart, it furnished very little fibrin and did not coagulate. When examined under the microscope, the globules appeared as perfectly round corpuscules of a dull white color, without any nucleus, and with a few dentated red rays along the edges.

The bicarbonate of potass developed the red colored rays on the corpuscles: the same effect was produced more evidently by a concentrated solution of sulphate of copper, and the nuclei now appeared.

On washing the corpuscles with water, they became of a dull white color, and seemed to pass gradually into a gelatinous, granular mass of molecules.

Concentrated acetic acid turned the corpuscles into molecules. At first, each globule changed into a cluster of small-granules, which soon separated, thus showing the difference between them and the globules of healthy blood.

Under a solution of chlorine the globules remained transparent, but soon changed into a granular mass. When concentrated muriatic acid was mixed with a few drops of the blood, a considerable quantity of the gas was disengaged, and the corpuscles were converted as before into molecules, with a few opaque, contracted globules mixed amongst them.

Treated with concentrated nitric acid the globules became small, round, and opaque; some few were oblong; some pointed at both ends.

Sulphuric ether, iodine, and corrosive sublimate, were also employed with analogous effects. The action of water, chlorine, acetic and muriatic acids, was altogether different from that produced on healthy blood; the three first substances rapidly changing the corpuscles into molecules, and the latter disengaging a remarkable quantity of gas. The peculiarities then observed in the blood in this case may be stated to be—dull white globules, without nuclei; no appearance of the crenated margin; a very quick transition of the corpuscles into molecules, and a tendency to lose their individual character. —*Prov. Med. Jour.*, July 2, 1842.

Asylum Journal.—A quarto sheet is hereafter to be published weekly at the Insane Asylum in Brattleboro', Vt., *by the inmates of the institution.* This is indeed an extraordinary age, when lunatics write for the amusement and instruction of the rest of mankind! Who can say, however, but that a large part of those who are outside the walls of insane hospitals are quite as crazy as those within? The contents of the first No. are creditable to the literary taste and morals of those who prepared it for the press. All the profits are to be applied to the support of the indigent residents of the asylum, of whatever name or denomination. The price is only one dollar a year, and those who profess to be the friends of unfortunate humanity cannot better appropriate that pittance than by forwarding it to Brattleboro' for the sustenance of this novel enterprise.—*Boston Med. and Surg. Jour.*

Case of Rupture of the Diaphragm in a Horse.—On the 8th of January last, Mr. Cope, whose pupil I have the honor to be, was desired to look at a black mare, the property of a farmer in this county, who said that, in coming up one of the steep hills that are so plentiful in the county, she stopped suddenly and breathed very laboriously, so that he could hear her at a great distance off. She was unable to proceed for full fifteen minutes, and then she slowly journeyed on with her load, which was upwards of a ton. On coming under our care the following symptoms were presented:—

She was constantly lying down and getting up again; while down she would frequently turn her head to her side; she perspired very much; the pulse was 80, and hard; the extremities moderately warm.

Venesection was had recourse to immediately, and copiously; and opium, combined with ol. lini, was administered; but in spite of our efforts she died in about six hours after the first appearance of illness.

It would be useless to occupy much time in describing the post-mortem appearances: suffice it to say, that on examining the chest the diaphragm was found to be ruptured. The rupture extended about seven inches, and a large portion of the intestines had protruded through the opening. They were very considerably inflamed, and this rupture was doubtless the cause of death.—*Mr. Osborne, in Veterinarian.*

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OURSELVES AND OUR ENTERPRISE.

This number completes the sixth volume of our Journal. It is with no ordinary feelings of regret, that, in looking back, we are compelled to admit that we have not made it equal, in variety and richness, to the expectations which its title, and the number of its editors, may have raised in the minds of its readers. But while making this confession, justice requires that we should remind them, and our brethren generally in the west, of their own delinquencies as writers; that in establishing a special department for their contributions, we had a right to expect their co-operation; and that one source of the barrenness to which we are confessing, is their own remissness. As we hope in future to do better ourselves, so we anticipate that they will display greater activity as contributors.

Our seventh volume will open with an elaborate experimental paper, by our indefatigable collaborator, Prof. Gross, on wounds of the intestines, illustrated with engravings. It will occupy a considerable portion of the original department of the first three numbers; and must, we think, be found acceptable to all who have a taste for patient and accurate inquiry.

A word to our correspondents. As two of us are often absent from Louisville during the vacations of our school, it is proper that communications for the Journal should be directed *To the Editors*, without naming any one, otherwise they may be sent away or remain unopened. In connexion with this suggestion, we wish to say that we have nothing to do with the business affairs of the Journal; and, therefore, that all letters relative to the loss of numbers in the mail, or to subscriptions, or enclosing money, should be directed to Messrs. *Prentice & Weissinger*, Publishers.

In conclusion, a hint to those who send us pamphlets. If any thing be written upon them, *the name, or respects, or any other salutations of the author or giver*, it subjects them to *letter* postage. Such is the present regulation of the Post Office Department; and, whether enforced generally or not, it is rigidly carried out in this city; whose Post Master, one of *our own profession*, has the integrity (of which we should feel proud) to execute, most faithfutly, the duties enjoined upon him by the government. EDS.

TRANSACTIONS OF THE ALUMNI SOCIETY OF THE UNIVERSITY OF NEW YORK.

The alumni of the College of Physicians and Surgeons of the State of New York, have issued a pamphlet of sixteen closely and neatly printed pages, giving an account of their first annual meeting and of their plan for future operations. This report embraces, for so small a publication, a great variety of exceedingly interesting matter, as will be seen by an inspection of the list of articles. The first paper is on the statistics of Fort Fairfield, Maine, by Dr. Coolidge of the U. S. A., and contains memoranda of the weather, facts and discussions concerning the pathology and treatment of delirium tremens, and valuable statistics relative to variola. The second article is an ingenious one, being an attempt at an explanation of the difference in size of the male and female urinary bladder; the third is on the mechanism of vomiting; the fourth gives an instructive case of aneurism of the femoral artery cured by ice. Our friend, Lewis A. Sayre, M. D., has an interesting case, in the next article, of abscess from pneumonia of the left lung, in a tuberculous patient, termina-

ting favorably, by forming a fistulous opening between the third and fourth ribs, and an abscess in the substance of the lung. A preparation was exhibited by the President of the Society illustrative of a case in which there had been caries of the dorsal vetebræ, and the discharge had communicated with the right pleural cavity. It was associated with tuberculous disease, and the matter was discharged by an opening between the fifth and sixth ribs. As long as the opening remained, the expectoration was trifling, but on its closure, profuse expectoration took place, and when the opening was again made, the expectoration ceased. After a second closure of the opening, the patient died, apparently suffocated by the immense quantity of fluid with which the bronchiæ became filled. In the autopsical examination, it was discovered that the lung had almost entirely disappeared. An elaborate and learned paper follows, on carcinoma in general, and cancer of the stomach. The statistics of this paper are full, and of a highly interesting character. Various morbid specimens were presented to the society, among the rest, one of the cranium of a young woman who had died of secondary syphilis, the whole parietal bone of which was riddled, the outer table being nearly destroyed; one of a lateral curvature of the spine, in which the dorsal stood at a full right angle to the lumbar vertebræ; and the cervical again at a like angle to the dorsal; one of a laceration of the cerebellum of a woman who had fallen, striking on the back of her head; and one of a calculus composed of xanthic oxide, from the cabinet of Professor Parker, being the fifth of the kind on record. The President announces that circular letters will be sent to the members of the association, asking their attention to certain specific objects of inquiry. As an example, he proposes the following as a subject for the investigation of the members: "Have you known any form of fever to occur in the neighborhood of decomposing animal matter? If so, can it be fairly attributed to such decomposition, and what is the nature of such disease?"

Y.

SUB-CONJUNCTIVAL OPERATION FOR CATARACT.

The inordinate and ever-present desire for something new, is the curse and reproach of French surgery and medicine, more especially of the former. *La nouveaute d'une opinion, d'une decouverte,* is as

irresistible in its attractions to the profession there, as a candle is to a moth. Read the following:

"M. Bernard, of Paris, has published the details of an operation for depression of a cataract, in which he had recourse to the subconjunctival proceeding. The patient, an aged female, was seated in a low chair, the upper eyelid raised by a strabismus levator, and the eye fixed by a hook passed into the sclerotica. The conjunctiva being then drawn upon by another hook, the needle was passed in until it reached the place of election in the sclerotica, when the conjunctival hook being withdrawn, the elasticity of the membrane prevented its interfering with the passage of the needle. The cataract was then depressed in the usual way, and the instrument withdrawn. On examining the eye afterwards, the wound in the conjunctiva could not be perceived until the patient was directed to look inwards, when a slight red point, twelve millimetres from the cornea, could be distinguished. The patient had neither pain nor other inconvenience after the operation, and recovered perfectly.—*Prov. Med. Jour.* from *Gaz. Medicale.*

"M. Bernard of Paris" would doubtless have been as profitably employed in whittling sticks with a penknife, Yankee fashion. And yet we should not be surprised, if, after having performed the above wonderful achievement, he exclaimed, in the fulness of his beatitude,

> Non omnis moriar: multaque pars mei
> Vitabit Libitinam. Usque ego posterâ
> Crescam laude recens,———

A subconjunctival method of depressing a cataract! Now, what end is proposed to be accomplished by it that is not attained by the ordinary method? Look, too, at the assemblage of instruments—a hook to hold the eyelids, a hook inserted into the sclerotic coat, a hook to catch up the mucous membrane! A man of genius unquestionably, this same "M. Bernard of Paris," if genius is evinced by the simplicity of means required to effect an end! truly, a most fingent man, if we may use a Carlyle-ism. Peradventure some would-be Roux, some embryo Lisfranc, some unfledged Velpeau, or some *veritable Bernard*, will ere long present us with a subcutaneous method of amputating a limb! It would be but a fitting cap to the climax of their absurdities. C.

ABUSE OF CALOMEL BY SOUTHERN PHYSICIANS.

The paragraph below has met our eye in several newspapers, and we believe it has quite made the circle of the Union. From what "lectures" it is taken, or how it got into the public prints, we are entirely ignorant.

"The following extract is from the lectures of N. Chapman, M. D., Professor of the Institutes and Practice of Medicine in the University of Pennsylvania, located in Philadelphia. He thus discourseth on the use of calomel:

Gentlemen—If you could only see what I almost daily see in my private practice in this city, persons from the south, in the very last stages of a wretched existence, emaciated to a skeleton, with both tables of the skull almost completely perforated in many cases, the nose half gone, with rotten jaws, ulcerated throats, breaths more pestiferous, more intolerable than poisonous upas, limbs racked with the pain of the inquisition, mind as imbecile as the puling babe's, a grievous burden to themselves, and a disgusting spectacle to others, you would exclaim, as I have often done, "Oh! the lamentable want of science that dictates abuse of that noxious drug, calomel, in the southern States!"

A horrible catalogue, indeed! a very lazar-house picture, fearful enough to contemplate, and embellished with the acknowleged skill of the author, but which, nevertheless, displays rather his imagination than his knowledge of the actual state of things. That southern physicians do use calomel in larger doses than their northern brethren, is certainly true; but so do they use quinine, opium, and many other potent articles. The reason is obvious: the diseases with which they have to contend, are rapid and dangerous to a degree entirely unknown further north, and their remedial measures have to be pursued with an energy and decision that northern diseases seldom require. Dr. Chapman need not be told that the susceptibility to remedies varies with climate, &c., and that a dose of medicine which would be all-sufficient in Philadelphia, would be wholly insufficient in New Orleans.

Now it is a little singular that while such evils as the above are charged against calomel, we never hear any thing of bad consequences arising from the other powerful and poisonous drugs we have named. We believe they would be just as well founded in the latter case as they are in the former. And we do not believe they are in either. We do not believe that mercury, when given for other diseases than those of venereal origin, will be followed by the effects here depicted: and the experience of a majority of the profession bears us out in this non-belief. In never a single instance have such effects been seen, save where the individual had led a suspicious life. Take a man who has never had syphilis, and let him be *drugged* with calomel till you are tired, and he will not have perforations of the cranium, nor ulcerations of the nasal bones, nor nocturnal pains, nor any of the thousand and one affections that are ascribed to it.

Our southern friends will no doubt feel exceedingly obliged to Prof. Chapman for his high opinion of their intelligence and scientific attainments. "Oh! the lamentable want of science that dictates abuse of that noxious drug, calomel, in the southern States!" A most pathetic exclamation it is, truly! The rebuke comes, too, with an excellent grace from the man who lent his name to give currency to Swaim's *Panacea!*—a *nostrum* which has done incalculable harm, and to which these lamentable effects, if they exist, are in many cases to be ascribed. And the unction attending this objurgation is still more remarkable, when we recollect that the physicians thus aspersed, received their medical education at the hands of the learned professor himself. It is a well-known fact that a very large majority of southern physicians are graduates of the University of Pennsylvania, and it is to be presumed that they use calomel and other articles as they were there taught to use them.

Granting, though, that this picture, with all its Titian-like coloring, is strictly true, there are other modes of expressing the same truth, not so obnoxious to censure. We go for telling the truth always, be it remembered, cut where it will; but the mode, the manner, may be a fair subject of consideration. Now we object to the mode here pursued, apart from the rank injustice it does to a numerous and highly respectable portion of the profession. The advocates of the various quack medicines and systems paraded through the country, will catch at this extract, as if it were a very god-send; changes will be rung upon it, and it will be emblazoned every where, much to the detriment of society. Many a one will be frightened at such a fearful array of consequences, and refuse, may be when life itself depends upon it, the employment of the indispensable drug. Would Dr. Chapman end his career as he began it, by playing into the hands of the veriest charlatans that ever went unwhipped of justice? We fain hope that no such sentiments have been expressed by him, and that the paragraph we have quoted is a forgery. C.

INDEX TO VOL. VI.

A

Abdominal tumors, 342.
Abuse of calomel by southern physicians, 474.
Alumni Society of the University of New York, transactions of the, 472.
Amaurosis, strychnia in, 397.
American Journal and Library of Dental Science, 61.
American Medicine before the Revolution, 149.
Amphoric rhonchus, researches into physical causes of, 145.
Amputations, circular and flap, comparative advantages of, 63.
Anatomy and Surgery, fall course of lectures on, 160.
Anatomy of the spleen, Bourgery on, 297.
Anatomy of fatty liver, 143.
Animal Chemistry, Liebig's, 342.
Antrum of Highmore, wound of, 186.
Asylum Journal, 470.
Auscultation, improvements in medicine from, 1, 80, 81.

B

Baron Larrey, 384.
Barry's Researches in Embryology, 257.
Bartlett on typhoid and typhus fever, 458.
Bell's Medical Library, 238.
Blood-globules, their formation and use, 463.
Bourgery on the anatomy of the spleen, 297.
Brain, compression of, 321.

C.

Cancerous affections of the womb, 380.
Carroll on scarlatina anginosa, 327.
Cartilages of the ribs, removal of—cure, 192.
Case of enormously enlarged stomach, 293.
Cases, miscellaneous, by Dr. Hogg, 252.
Catalogue and Circular of the Medical Department of Laporte University, 460.
Cataract, subconjunctival operation for, 473.
Calomel, abuse of by southern physicians, 474.
Chailley and Devilliers on auscultation in pregnancy, 392.
Chemistry, Liebig's Animal, 342.
" Kane's Elements of, 398.
Children at the breast, dentition of, 215.
Chronic spinitis, case of, 323.
Circular and flap amputations, comparative advantages of, 63.
Clinical lecture on delirium tremens, 196.
College of professional teachers, 79.
College, medical, report on, 235, 313.
Coloration of the bones by madder, 465.
Comparative frequency of tuberculous disease, 67.
Compression of the brain, 321.
Contraction of the fingers, 223.
Correction, 80.
Counter-irritants, 305.
Creasote, styptic effects of, 383.
Croton oil, external application of, 468.

D

Death of Dr. Samuel Hogg, 151.
Delirium tremens, clinical lecture on, 196.
Dentition of children at the breast, 215.
Devilliers and Chailley on auscultation in pregnancy, 392.
Diabetes mellitus, 69.
Diagnosis of gonorrhœa in accusations of rape, 147.
Diaphragm, rupture of in a horse, 470.
Diplomas, Latin, 236.
Diseases of Stark County, Ohio, 156.
Diseases of Rutherford County, Tennessee, 426.
Disease of the kidney, 462.
Disease, tuberculous, comparative frequency of, 67.

Discoloration of the skin from nitrate of silver, removal of, 395.
Division of muscles in scrivener's spasm, 190.
Donne on wound of antrum of highmore, 186.
Double uterus, physiological observations on, 467.
Dunglison's Medical Library, 238.

E

Effects of the times on the profession, 189.
Elements of Chemistry, Kane's, 398.
Eements of Surgery, Gross's edition of Liston's, 453.
Embryology, Barry's researches in, 257.
Enlarged stomach, 293.
Examination, post mortem, of an old soldier, 393.
Experiments on mesmerism, 71.
" kiesteine, 206.
Expulsion of a mass of hair from the uterus, 217.
External application of croton oil, 468.
Extract of belladonna, in sciatica, 394.
Eyelid, cysts of, 311.

F

Face, gun-shot wound of, 230.
Facts and conjectures on the trembles and milk-sickness, 181.
Fall course of lectures on anatomy and surgery, 160.
Fatty degeneration of liver, 144.
" " minute anatomy of, 143.
Femoral aneurism, ligature of external iliac for, 299.
Fever, yellow, of Natchez and the southwest, 161.
Fever, typhoid and typhus, Bartlett on, 458.
Fever, typhoid, Louis on, 386.
Fingers. permanent contraction of, 223.
Flap and circular amputations, compared, 63.
Fluor spar, 313.
Formation and use of blood-globules, 463.
Fractures, old, division of tendons in, 147.
Fracture of the tibia—necrosis—resection, 65.

G

Gonorrhœa, diagnosis of, in accusations of rape, 147.
Gross' edition of Liston's Elements of Surgery, 453.
Gun-shot wound of the face, 230.

H

Hair, mass of, expelled from the uterus, 217.
Hemorrhage, secondary, after amputation, 63.
History, diagnosis and treatment of typhoid and typhus fever, 458.
Hogg, Dr. Samuel, death of, 151.
" " miscellaneous cases of, 252.
Homœopathy, 319.
Hydrocele, new mode of treating, 194.
Hydrophobia, state of the blood in, 469.

I

Incipient stage of cancerous affections of the womb, 380.
Incomes of professional men in London, 296.
Influence of the nerves on muscular irritability, 193.
Injuries of the superior extremities, 241.
Injury to cartilages of the ribs—removal—cure, 192.
Improvements in practical medicine from auscultation, 1, 80, 81.
Instruction, private medical, 160, 239.
Introduction of syphilis into Scotland, 196.
Invalids of the south, summer resort for, 401.

J

Journal and Library of Dental Science, 61.
Journal of Medicine and Surgery, New England Quarterly, 59.

K

Kane's Elements of Chemistry, 398.
Kidney, disease of the, 462.
Kiesteine, experiments on, 206.
Kreasote, styptic effect of, 383.

L

Lakes of the north as a summer residence, 401.
Lancet, Western, 158.
Laporte University, catalogue and circular of, 460.
Larrey, death of, 384.
Latin diplomas, 236.
Lectures on anatomy and surgery, fall course of, 160.
Lecture on delirium tremens, 196.
Liebig's Animal Chemistry, 342.
Ligature of the primitive iliac, 390.
" external iliac, 299.
Lime moxa, 194.
Liston's Elements of Surgery, 453.
Liver, fatty degeneration of, 143, 144.
Louis on typhoid fever, 386.

M

Madder, coloration of the bones by, 465.
Maleolus internus, avulsion of, with fracture of tibia, 65.
Mass of hair, expelled from the uterus, 217.
Medicine, American, state of before the Revolution, 149.
Medicine and Surgery, New England Quarterly Journal of, 59.
Medical instruction, private, 160, 239.
Medical Society of Temessee, 150.
Medical College, report on, 235, 313.
" appointments, 239.
Metallic tinkling, physical causes of, 145.
Milk-sickness, facts and conjectures on, 181.
Miscellaneous cases, by Dr. Hogg, 252.
Mode of preserving nitrate of silver, 463.
Monette on yellow fever, 161.
Moxa of lime, 394.
" in rheumatism, 309.
Muscular irritability, influence of nerves on, 193.
Muscles, division of for scrivener's spasm, 190.

N

Napoleon and phrenology, 399.
Natchez, yellow fever of, 161.
Nature and treatment of scrofula, 468.
Necrosis of tibia, resection, &c., 65.
Nerves, influence of on muscular irritability, 193.
New England Quarterly Journal of Medicine and Surgery, 59.
New mode of treating hydrocele, 194.
New theory of tinea, &c., 228.
Nitrate of silver, mode of preserving, 217.
Northern lakes, a summer residence for southern invalids, 401.

O

Observations on the epidemic yellow fever of Natchez, 161.
Observations on scarlatina anginosa, 327.
Operation for cataract, subconjunctival, 473.
Ourselves and our enterprise, 471.

P

Paraphymosis, treatment of, 148.
Paraplegia, ergot of rye in cases of, 395.
Permanent contraction of the fingers, 223.
Phrenology and Napoleon, 399.
Phrenological visit to penitentiary for young criminals. 233.
Physical causes of metallic tinkling, 145.
Physiological observations on double uterus, 467.
Post-mortem examination of an old soldier, 393.
Practical medicine, influence of auscultation on, 1, 80, 81.
Pregnancy, auscultation in diagnosis of, 392.
Primitive iliac artery, ligature of, 390.
Private medical instruction, 160, 239.
Professional incomes in London, 296.
Professional teachers, college of, 79.
Prolapsus and separation of the vagina, 191.

R

Rape, diagnosis of gonorrhœa in accusations of, 147.
Report on the Medical College, 235, 313.
Researches in embryology, Barry's, 257.
" " into physical causes of metallic tinkling, 145.
Rupture of the diaphragm in a horse, 470.

S

Scarlatina anginosa, at St. Clairsville, Ohio, 327.
Sciatica cured by extract of belladonna, 394.
Scotland, first introduction of syphilis into, 196.
Scrivener's spasm, cured by division of muscles, 190.
Scrofula, nature and treatment of, 468.
Secondary hemorrhage after amputations, &c., 63.
Separation and prolapsus of vagina, 191.
Small-pox pustule, structure of, 219.
Southern invalids and northern lakes, 401.
Spinitis, chronic, Smith's case of, 323.
Spleen, Bourgery on anatomy of, 297.
South-west, epidemic yellow-fever of, 161.
State of American medicine before the Revolution, 149.
State of the blood in hydrophobia, 469.
Stomach, enormously enlarged, 293.
Strychnia in amaurosis, 397.
Styptic effect of creasote, 383.
Subconjunctival operation for cataract, 473.
Superior extremities, injuries of, 241.
Surgery, Liston's Elements of, 453.

T

Teachers, professional, college of, 79.
Tibia, fracture of—necrosis, resection, recovery, 65.
Times, effects of, on the profession, 189.
Tinea, new theory of—its vegetable origin, 228.
Tinkling, metallic, physical causes of, 145.
Transactions of the Alumni Society of the University of New York, 472.

Treatment of old fractures by division of tendons, 147.
Treatment of paraphymosis, 148.
Trembles and milk-sickness, 181.
Tuberculous disease, comparative frequency of, 67.
Tumors, abdominal, 306.
Typhoid and typhus fever, Bartlett on, 458.
Typhoid fever, Louis on, 386.

U

University of Virginia, 236.
Use of moxa in rheumatism, 309.
Uterus, cancerous affections of, 380.
Uterus, double, physiological observations on, 467.

V

Vagina, prolapsus and separation of, 191
Vegetable origin of tinea, 238.
Velpeau on cysts of the eyelid, 311.
Virginia, University of 236.

W

Western Lancet, 158.
Wilson's case of compression of the brain, 321.
Womb, incipient stage of cancerous affections of, 380.
Wound of the antrum of Highmore, 186.
Wound, gun-shot, of the face, 230.

Y

Yellow fever of Natchez and the south-west, 161.

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